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A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

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VOL. XVII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR; FOR

THE AUTHOR,

AND SOLD BY

J. STRATFORD, No. 112, HOLBORN-HILL.

1807.

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PREFACE.

THE Authors of Original Papers and Communications in the present Volume are, W. H. Wollaston, Sec. R. S.; Mr. W. Skrimshire, Jun.; W. W.; Mr. John Tatum, Jun.; Emeritus; R. T.; W. X.

Of Foreign Works, C. A. Prieur; C. L. Cadet; M. Desormes; M. Clement; M. Proust; M. Bouillon Lagrange; Dr. Samuel L. Mitchell; M. Antony Alexis Cadet de Vaux; M. Vauquelin; M. Antony Thillaye-Plat; F. F. Delaroche; M. Lamarck; M. De Lalande; M. Laplace; John Michael Haussman; M. Guyton; Dr. Haldat; M. Henry; M. Darso; M. Gehlen; M. Erman; M. Delaville; M. Dupont de Nemours; M. Planche; M. And. de Gy; M. Chaptal; M. Germon; C. H. Pfaff; M. J. J. Champollion Figeac; M. Roswag; M. Bellemère; Messrs. Von Humboldt and Gay-Lussac; M. Cotte; M. Carnot.

And of British Memoirs abridged or extracted, Benjamin Count of Rumford; Richard Phillips, Esq.; Dr. W. Roxburg; Mr. Charles Wilson; Mr. Robert Salmon; Mr. John Austin; Mr. Jessop; M. G. Field; J. Curwen, Esq. M. P.; Mr. Charles Waistell; Mr. John Trotter; Mr. James Hardie; Rev. James Headrick.

The Engravings consist of 1. Camera Lucida, by W. Hyde Wollaston, M. D. Sec. R. S.; 2. Decomposition of Light, by C. A. Prieur; 3, 4. Apparatus of Mr. Thillaye Platel for the Carbonization of Turf; 5. Mr. Tatum's Apparatus for ascertaining the Increase of Temperature by the Galvanic Action; 6. Theory of Looming, or Horizontal Refraction; 7. Mr. Salmon's Specimens of good and bad Pruning of Fir Trees; 8. Mr. Field's Stove for heating or drying; 9. Mr. Waistell's Horse Hoe; 10. Mr. Curwen's Weed Harrow; 11. Apparatus for triturating Quicksilver; 12. Erman's new Classes of Galvanic Conductors; 13. Mr. J. Trotter's Curvilinear Saw; 14. Mr. J. Hardie's Bookbinder's Cutting Press.

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A

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THE ARTS.

JUNE, 1807.

ARTICLE I.

Description of the Camera Lucida. By W. H. Wollaston, *Sec. R. S.*

New instrument for delineation.

HAVING a short time since amused myself with attempts to sketch various interesting views, without an adequate knowledge of the art of drawing, my mind was naturally employed in facilitating the means of transferring to paper the apparent relative positions of the objects before me; and I am in hopes that the instrument, which I contrived for this purpose, may be acceptable even to those who have attained to greater proficiency in the art, on account of the many advantages it possesses over the *Camera Obscura*.

Principles.

The principles on which it is constructed will probably be most distinctly explained by tracing the successive steps, by which I proceeded in its formation.

Objects seen as if transferred to paper, by reflection from transparent glass.

While I look directly down at a sheet of paper on my table, if I hold between my eye and the paper a piece of plain glass, inclined from me downwards at an angle of 45°, I see by reflection the view that is before me, in the same direction that I see my paper through the glass. I might then take a sketch of it; but the position of the objects would be reversed.

By a single reflection are inverted;

To obtain a direct view, it is necessary to have two reflections.

two reflections give the natural position.

flections. The transparent glass must for this purpose be inclined to the perpendicular line of sight only the half of 45°, that it may reflect the view a second time from a piece of looking glass placed beneath it, and inclined upwards at an equal angle. The objects now appear as if seen through the paper in the same place as before; but they are direct instead of being inverted, and they may be discerned in this manner sufficiently well for determining the principal positions.

The objects and the paper cannot be distinctly seen at once.

The pencil, however, and any object, which it is to trace, cannot both be seen distinctly in the same state of the eye, on account of the difference of their distances, and the efforts of successive adaption of the eye to one or to the other, would become painful if frequently repeated. In order to remedy this inconvenience, the paper and pencil may be viewed through a convex lens of such a focus, as to require no more effort than is necessary for seeing the distant objects distinctly. These will then appear to correspond with the paper in *distance* as well as *direction*, and may be drawn with facility, and with any desired degree of precision.

Arrangement of the glasses.

This arrangement of glasses will probably be best understood from inspection of Fig. I. *a b* in the transparent glass; *b c* the lower reflector; *b d* a convex lens (of twelve inches focus) *e* the position of the eye; and *f g h e* the course of the rays. See Pl. I.

Another construction.

In some cases a different construction will be preferable. Those eyes, which without assistance are adapted to seeing near objects alone, will not admit the use of a convex glass; but will on the contrary require one that is concave to be placed in front, to render the distant objects distinct. The frame for a glass of this construction is represented at *i k*, (fig 3.) turning upon the same hinge at *h* with a convex glass in the frame l m, and moving in such a manner, that either of the glasses may be turned alone into its place, as may be necessary to suit an eye that is long or short sighted. Those persons, however, whose sight is nearly perfect, may at pleasure use either of the glasses.

Difference in the latter in-

The instrument represented in that figure differs moreover in other respects from the foregoing, which I have chosen

to describe first, because the action of the reflectors there employed would be more generally understood. But those who are conversant with the science of optics will perceive the advantage that may be derived in this instance from prismatic reflection; for when a ray of light has entered a solid piece of glass, and falls from within upon any surface, at an inclination of only twenty-two or twenty-three degrees, as above supposed, the refractive power of the glass is such as to suffer none of that light to pass out, and the surface becomes in this case the most brilliant reflector that can be employed. strument. Prismatic reflection.

Fig. 2. represents the section of a solid prismatic piece of glass, within which both the reflections requisite are effected at the surfaces *a b*, *b c*, in such a manner that the ray *f g*, after being reflected first at *g*, and again at *h*, arrives at the eye in a direction *h e* at right angles to *f g*. Figure of the prism.

There is another circumstance in this construction necessary to be attended to, and which remains to be explained. Where the reflection was produced by a piece of plain glass, it is obvious that any objects behind the glass (if sufficiently illuminated) might be seen through the glass as well as the reflected image. But when the prismatic reflector is employed, since no light can be transmitted directly through it, the eye must be so placed that only a part of its pupil may be intercepted by the edge of the prism, as at *e* Fig. 2. The distant objects will then be seen by this portion of the eye, while the paper and pencil are seen past the edge of the prism by the remainder of the pupil. Remedy for the want of transmission of direct light through the prism.

In order to avoid inconvenience that might arise from unintentional motion of the eye, the relative quantities of light to be received from the object, and from the paper are regulated by a small hole in a piece of brass, which by moving on a center at *c*, fig. 3. is capable of adjustment to every inequality of light that is likely to occur.

Since the size of the whole instrument, from being so near the eye, does not require to be large, I have on many accounts preferred the smallest size that could be executed with correctness, and have had it constructed on such a scale, that the lenses are only $\frac{1}{2}$ of an inch in diameter. The instrument is of small dimensions.

Though the original design, and principal use of this instrument It may be used in copying drawings.

strument is to facilitate the delineation of objects in true perspective, yet this is by no means the sole purpose to which it is adapted; for the same arrangement of reflectors may be employed with equal advantage for copying what has been already drawn, and may thus assist a learner in acquiring at least a correct outline of any subject.

nstructions.

For this purpose the drawing to be copied should be placed as nearly as may be at the same distance before the instrument that the paper is beneath the eye-hole, for in that case the size will be the same, and no lens will be necessary either to the object, or to the pencil.

It answers every purpose of the pentagraph.

By a proper use of the same instrument, every purpose of the pentagraph may also be answered, as a painting may be reduced in any proportion required, by placing it at a distance in due proportion greater than that of the paper from the instrument. In this case a lens becomes requisite for enabling the eye to see at two unequal distances with equal distinctness, and in order that one lens may suit for all these purposes, there is an advantage in varying the height of the stand according to the proportion in which the reduction is to be effected.

Method of using the instrument for this purpose.

The principles on which the height of the stem is adjusted will be readily understood by those who are accustomed to optical considerations. For as in taking a perspective view the rays from the paper are rendered *parallel*, by placing a lens at the distance of its *principal* focus from the paper, because the rays received from the distant objects are *parallel;* so also when the object seen by reflection is at so short a distance that the rays received from it are in a certain degree *divergent*, the rays from the paper should be made to have the same degree of divergency in order that the paper may be seen distinctly by the same eye; and for this purpose the lens must be placed at a distance less than its principal focus. The stem of the instrument is accordingly marked at certain distances to which the conjugate foci are in the several proportions of 2, 3, 4, &c. to 1, so that distinct vision may be obtained in all cases, by placing the painting proportionally more distant.

Magnified designs.

By transposing the convex lens to the front of the instrument and reversing the proportional distances, the artist might also enlarge his smaller sketches with every desirable degree

Camera lucida.

By W. Hyde Wollaston, M.D. Sec. R.S.

Fig. 1.



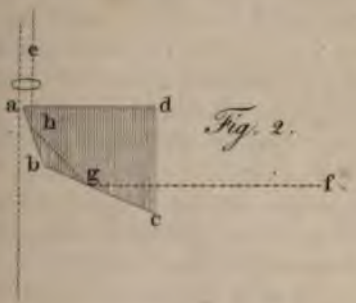


Fig. 2.

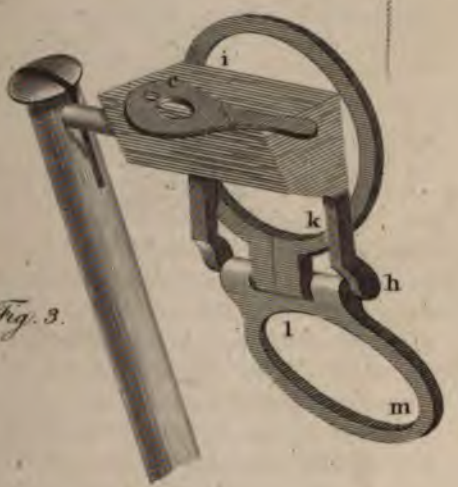


Fig. 3.

degree of correctness, and the naturalist might delineate minute objects in any degree magnified.

Since the primary intention of this instrument is already, in some measure, answered by the *Camera Obscura*, a comparison will naturally be made between them.

The objections to the Camera Obscura are

1st. That it is too large to be carried about with convenience. Comparison of the camera lucida with the camera obscura.

The Camera lucida is as small and portable as can be wished.

2dly. In the former, all objects that are not situated near the centre of view are more or less distorted.

In this, there is no distortion; so that every line, even the most remote from the centre of view, is as strait as those through the centre.

3dly. In that, the field of view does not extend beyond 30° or at most 35° with distinctness.

But in the *Camera Lucida* as much as 70° or 80° might be included in one view.

As it has been thought advisable to secure an exclusive sale of this instrument by patent, those who are desirous of purchasing it are informed that Mr. Newman (No. 24 Soho Square) has at present the disposal of it.

II.

Description of a new Boiler constructed with a View to the saving of Fuel. By BENJAMIN COUNT OF RUMFORD. *Read at a Meeting of the first Class of the National Institute the 6th October*, 1806*.

IT is well known that much is gained in the saving of fuel, when an extensive surface is given to that part of the boiler against which the flame strikes, but this advantage is often counterbalanced by great inconveniences. For a boiler of the form usually employed, having the bottom very much extended in proportion to its capacity, must necessarily present a great surface to the atmosphere, and the loss of heat, occasioned by the cold air coming in contact with Boiler for generating steam; having its bottom terminating in tubes.

* Translated by W. Caddel Esq. and revised by the count, from whom it was received.

this

Boiler for generating steam; having its bottom terminating in tubes.

this surface, may be more than sufficient to compensate the advantage derived from the extended surface of the bottom. And where the boiler is employed for producing steam, as it is indispensably necessary that it should be of a thickness sufficient to resist the expansive force of the steam, it is evident, that if the diameter be augmented (with a view to increase the surface of the bottom) a considerable expence is incurred on account of the additional strength that must be given to the sides.

Having been engaged in the year 1796, in a set of experiments, in which I employed the steam of boiling water as a vehicle of heat; I had a boiler made for this purpose, on a new construction, which answered well, and even beyond my expectations; and, as this boiler might be used with advantage in many cases, even where it is only required to heat liquids in an open boiler, this, and another motive, which it would be useless to mention in this place, have lately induced me to construct one here (at Paris) and to present it to the Institute.

The object chiefly had in view in the construction of this boiler, was to give it such a form, that the surface exposed to the fire should be great in comparison with its diameter and capacity; and this without having a great surface exposed to the cold air of the atmosphere.

The body of the boiler is in the shape of a drum. It is a vertical cylinder of copper twelve inches in diameter, and twelve inches high, closed at top and at bottom by circular plates.

In the centre of the upper plate there is a cylindrical neck six inches in diameter, and three inches high, shut at top by a plate of copper three inches in diameter and three lines in thickness, fastened down by screws.

This last plate is pierced by three holes, each about five lines in diameter. The first, which is in the center of the plate, receives a vertical tube, which conveys water to the boiler from a reservoir which is placed above. This tube, which descends in the inside of the boiler, to within an inch above the circular plate which forms its bottom, has a cock near its lower end. This cock is alternately opened and

and shut, by means of a floater which swims on the surface of the water contained in the body of the boiler.

Boiler for generating steam; having its bottom terminating in tubes.

The second of the holes in the plate that closes the neck of the boiler, receives the lower end of another vertical tube, which serves to convey the steam from the boiler to the place where it is to be used.

The third hole is occupied by a safety valve.

This description shews that there is nothing new in the construction or arrangement of the upper part of this boiler. In its lower part there is a contrivance for increasing its surface, which has been found very useful.

The flat circular bottom of the body of the boiler, which as I said before is twelve inches in diameter, being pierced by seven holes, each three inches in diameter, seven cylindrical tubes of thin sheet copper, three inches in diameter, and nine inches long, closed below by circular plates, are fixed in these holes, and firmly rivetted, and then soldered to the flat bottom of the boiler.

On opening the communication between the boiler and its reservoir, the water first fills the seven tubes, and then rises to the cylindrical body of the boiler; but it can never rise above six inches in the body of the boiler, for when it has got to that height, the floater is lifted to the height necessary for shutting the cock that admits the water.

When the height of the water in the boiler is diminished a few lines by the evaporation, the floater descends a little, the cock is again opened, and the water flows in again from the reservoir.

As the seven tubes that descend from the flat bottom of the body of this boiler into the fire place, are surrounded on all sides by the flame, the liquid contained in the boiler is heated, and made to boil in a short time, and with the consumption of a relatively small quantity of fuel; and when the vertical sides of the body of the boiler, and its upper part are suitably enveloped, in order to prevent the loss of heat by these surfaces, this apparatus may be employed with much advantage in all cases where it is required to boil water for procuring steam.

And as in the case where the boiler is constructed on a great scale, the seven tubes that descend from the bottom of

Boiler for generating steam, having its bottom terminating in tubes.

of the boiler into the fire may be made of cast iron, whilst the body of the boiler is composed of sheet iron, or sheet copper; it is certain that a boiler of this kind, sufficiently large for a steam engine, a dying house, or a spirit distillery, would cost much less than a boiler of the usual form, of equal surface and power.

But in all cases where it is required to produce a great quantity of steam, it will be always preferable to employ several boilers of a midling size, placed beside each other, and heated each by a separate fire, instead of using one large boiler heated by one fire.

I have shewn, in my sixth essay, on the management of fire, and the œconomy of fuel, that beyond a certain limit, there is no advantage derived from augmenting the capacity of a boiler.

It will be perceived, that the boiler which I have the honour of presenting to this Society, is of a form fit for being placed in a portative furnace, and it was actually intended for that purpose.

Its furnace, which is made of bricks, with a circular iron grate of six inches in diameter, is built in the inside of a cylinder of sheet iron, seventeen inches in diameter, and three feet high, and can be easily transported from place to place, by two men.

This cylinder of sheet iron, which is divided into two parts, in order to facilitate the construction of the masonry, weighs only forty-six pounds. The masonry weighs about a hundred and fifty pounds, and the boiler twenty-two pounds.

In order to form an estimate of the advantage which the particular form of this boiler gives it in accelerating its heating, we may compare the extent of surface that it presents to the action of the fire, with that of the flat bottom of a common boiler.

The diameter of the bottom of a cylindrical boiler being twelve inches, the surface is 113.88 square inches; but the surface of the sides of the seven tubes that descend from the flat bottom of our boiler (which is likewise twelve inches in diameter) is 593.76 square inches. Therefore, the new boiler has a surface exposed to the direct action of the

the fire, more than five times greater than that of a boiler of equal diameter, and of the ordinary form: how much this difference must affect the celerity of heating is easy to conceive.

Boiler for generating steam; having its bottom terminating in tubes.

In the manner in which boilers are usually set, their vertical sides are but little struck by the flame, and on that account, I have not taken the effect of the sides into consideration in my estimate; but even taking them into account, the new boiler will always have a surface exposed to the fire, at least twice as great as that of a common cylindrical boiler of the same diameter, as can easily be shewn.

The new boiler being twelve inches in diameter, and twelve inches high, and each of its seven tubes being three inches in diameter, and nine inches high, its surface is 1160.44 square inches, without reckoning the circular plate that closes its top, nor its neck.

The surface of the bottom and sides of a cylindrical boiler of twelve inches in diameter, and twelve inches high, will be 566.68 square inches.

As the quantity of heat that enters a boiler in a given time, is in proportion to the extent of surface that the boiler presents to the fire, it is evident, that other circumstances being the same, a boiler with tubes descending from its bottom, will be heated at least twice as soon as a cylindrical boiler of the same diameter, with a flat bottom.

In order that a cylindrical boiler with flat bottom, surrounded by flame on all sides, might have the same extent of surface exposed to the fire as a boiler with tubes, it would be necessary to give it a diameter greater than that of the boiler with tubes in the proportion of the square root of 1160.44, to the square root of 566.68, that is, of 17.171 to 12.

Therefore, in order that a cylindrical boiler with a flat bottom, might have the same extent of surface exposed to the fire as our boiler with tubes, of twelve inches in diameter, it would be necessary to give it a diameter of 17.171 inches.

But if the diameter of a boiler intended for producing steam be increased, it is necessary, at the same time, to increase its thickness, in order to increase its strength.

The

Boiler for generating steam; having its bottom terminating in tubes.

The necessary increase of thickness, and the expence that it will occasion, can be easily calculated.

The effort that an elastic fluid exerts against the sides of the containing vessel, is in proportion to the surface of a longitudinal and central section of the vessel, and consequently in proportion to the square of its diameter, the form remaining the same. Hence we may conclude, that a steam boiler of a cylindrical form with a flat bottom, which has the same extent of surface exposed to the fire as a boiler of twelve inches in diameter with tubes, should be at least twice as thick as this last, in order to have an equal degree of strength for resisting the expansive power of the steam.

The boiler which I have the honour of presenting to the Society, is particularly intended to serve as a steam boiler, but it may undoubtedly be applied to other purposes. Having shewn it to M. Auzilly, son of a considerable soap manufacturer of Marseilles, he thought that it might be employed with advantage in the making of soap; and from what he told me of the process, and of the boilers employed in that art, I am persuaded that the experiment would succeed perfectly.

But after all, it remains to be determined, whether it would not be still more advantageous to employ steam as a vehicle of heat in the making of soap, instead of lighting the fire under the bottom of the vessel in which the soap is made.

The result of an experiment which we are to make, M. Auzilly and myself, will probably throw some light upon this question.

III.

Notice of an Experiment on the Use of the Heat of Steam, in Place of that of an open Fire, in the making of Soap, By Benjamin Count of Rumford. *Read at a Meeting of the First Class of the National Institute, the 20th of October,* 1806.

Experiment shewing the advantage of

I HAD the honour of announcing to this assembly, at the last meeting but one, that M. Auzilly and myself, were to make

make an experiment on the use of steam in the making of soap. This experiment we have made, and with perfect success. heating soap lees by steam.

I have the honour to lay before the Society, a piece of soap of about ten cubic inches, made in my laboratory by this new process, which required only six hours of boiling, whereas sixty hours and more are necessary in the ordinary method of making soap.

From all the appearances that we observed in the course of this experiment, and from its results, we think ourselves authorised to conclude, that this new method of making soap cannot fail to be advantageous in every respect, and that it will soon be generally adopted.

We propose to repeat the experiment on a larger scale, as soon as we shall be able to procure the necessary utensils, and we beg the Society to appoint commissioners to be present during its execution.

As I intend to communicate to the Institute, upon a future occasion, all the details of our experiment, with an account of the apparatus we employed in it, I shall for the present make only one observation on the probable cause of the acceleration of the formation of soap, which we observed. I believe that this acceleration is due, in great measure, if not entirely, to a motion of a peculiar kind in the mixture of oil and lye, occasioned by the sudden condensation of the steam introduced into the liquor. It is a sharp stroke, like that of a hammer, which made the whole apparatus tremble.

These strokes, which succeeded rapidly in certain circumstances, and which were violent enough to be heard at a considerable distance, must necessarily have forced the particles of oil and alkali to approach each other, and consequently to unite.

As the violence of these strokes diminished greatly as soon as the liquid had acquired nearly the temperature of the steam, I propose to supply this defect by a particular arrangement of the apparatus in the experiment we are going to make. I shall divide the vessel into two parts, by a horizontal diaphragm of thin sheet copper, and causing a slow current of cold water to pass through the lower division

division or compartment of the vessel, I shall introduce steam into it, through a particular tube destined for that purpose, as soon as the mixture of oil and alkali which occupies the upper division of the vessel is become too hot for condensing the steam.

The steam which enters the water (always kept cold) that fills the lower compartment of the vessel, will be condensed suddenly, and the sharp strokes which result will be communicated through the thin diaphragm to the hot liquid contained in the upper division of the vessel, and will, I expect, accelerate the union of the oil with the alkali. I shall then shut almost entirely the cock which admits steam into the upper division of the vessel, in order to prevent an useless consumption of steam and heat.

I shall not fail to give an account of the results of this new experiment to this assembly; and I shall rejoice if by any researches I shall be so happy as to contribute to the improvement of an art which is undoubtedly of great importance to society.

IV.

On the Habitudes of Saline Bodies with Regard to Electricity. By Mr. WILLIAM SKRIMSHIRE, *Jun. Communicated by Mr.* CUTHBERTSON.

DEAR SIR,

HAVING made some further progress in my electrical experiments, I take the liberty of sending you the results, in order, if you think proper, for insertion in Mr. Nicholson's valuable Journal.

WM. SKRIMSHIRE, JUN.

To Mr. CUTHBERTSON.

Saline Substances.

Causes of error in taking the spark from saline crystals.

WHEN the spark is taken from saline substances placed upon the conductor, there is some difficulty in ascertaining, whether the spark proceeds from the salt itself, or from the conductor, through the substance, or along the surface of the salt. A large crystal will sometimes appear to give a very

very brilliant spark ramified upon its surface; but if these ramifications be minutely examined, they will be found to proceed from the conductor, and running up the sides of the crystal converge to a point under the knob of the discharger.

Whenever it is doubtful whether a crystal affords a spark or not, I place a second crystal upon the first, and then apply the discharger to the uppermost, when in general, merely a hissing stream of electric light, or at most, a small hissing spark is perceived. Again, if the crystal be thin, it will appear to give as good a spark as any metal, and in truth the spark really proceeds from the conductor, and passing through the salt renders it transparent, or rather semi-transparent. A thin cake of agglutinated crystals allows the spark to pass through its interstices with the same appearance. It is necessary here to remark, with respect to passing the shock through saline substances, that if the salt be crystallized in large lumps like alum or borax, it is shivered in pieces by the shock; and the same happens when the lump consists of a congeries of regular crystals; but if the shock (meaning the shock which I constantly employ in these experiments, and which is never more than that from a quart phial) be passed through a single crystal, no such effect occurs, for the crystal remains perfectly whole, and is generally rendered luminous throughout, should the salt prove phosphoric by the electric light. Remedies, &c.

Alkalies and their Compounds.

Sub-carbonate of potash. 1st. Pearl ash gives a dense purple stream of electric light, instead of a spark, and is extremely phosphoric by the shock, its light continuing some minutes. 2nd. Salt of tartar is very luminous, and its particles are easily scattered by the shock, when the points of the dischargers are in contact with it. Alkali and alkaline salts.

Super-carbonate of potash in small crystals is luminous, and scattered about, if the rods touch it; in larger crystals it is also luminous, but they are not fractured by passing the shock through them.*

* I am inclined to suspect that the shock does not pass through, but over the surface of a single crystal, if it be a small crystal.

Sulphate

Alkali and alkaline salts.

Sulphate of potash affords a small spark, and is luminous by the shock.

Nitrate of potash, commonly called nitre or salt-petre, affords a spark which is beautifully flame-coloured on its surface; it is also luminous, but its light is of short duration.

Muriate of potash is much more phosphorescent than nitre, and its light is of longer continuance.

Hyper-oxymuriate of potash is lumious, but does not explode when the shock is passed through it.

Acidulous oxalate of potash is luminous by the electric shock.

Acidulous tartrite of potash. Red and white argol, and purified crystallized cream of tartar afford similar results, except that cream of tartar is rather more luminous than the others. They do not give a spark; but a cake of conglomerated crystals allows a spark to pass through it, rendering it almost transparent.

Neutral tartrite of potash, or soluble tartar of the apothecaries, is rendered luminous by the explosion.

Tartrite of potash and soda, or Rochelle salt of the apothecaries, affords a beautiful spark, flame coloured, and ramified upon the surface, when a single crystal is made use of; but when one crystal is placed upon another, and the discharger applied to the uppermost, only a purple hissing stream or a very slight spark can be taken from it. It is luminous when the shock is passed *above* its surface.

Acetite of potash gives a purple spark, flame coloured and ramified on its surface, even when one crystal is placed upon another. It is rendered luminous merely by taking the spark from the conductor near it; and by exposure to the shock it is beautifully phosphorescent, shining with a green light, and is even superior in brilliancy to the sulphuret of lime, though its light is of shorter duration than in that preparation. This salt is extremely deliquescent, in which state, and even when quite dissolved, it is still rendered luminous by exposure to the light of the explosion.

Soda. Sub-carbonate of soda affords a hissing purple spark, flame coloured on its surface. It is phosphorescent by the shock.

Sulphate

Alkali and alkaline salts.

Sulphate of soda gives a purple spark, and is luminous by the shock.

Nitrate of soda is not at all luminous, even when the points of the discharging rods are in contact with it, during the explosion.

Muriate of soda. Several native specimens of different colours were tried, and gave only a small stream of electric light, almost without any sound. They were all luminous by the shock, as was also common culinary salt.

Phosphate of soda affords only a purple hissing stream, but is luminous by the shock, though in a less degree than the sub-carbonate.

Sub-borate of soda. 1st. Tincal gives a purple hissing spark, apparently proceeding from a red point upon its surface. It is but slightly luminous by the shock. 2nd. East Indian borax gives no spark, but the electric fluid glides silently over every part of its surface from the conductor to the knob of the discharger. Its phosphorescency by the shock is superior to tincal, but not equal to the refined borax of the shops. 3rd. Refined borax affords a hissing purple spark, sometimes flame coloured upon its surface; but when a *large* piece is placed on the conductor it allows only a purple stream to be drawn from it. It is luminous when the explosion is made *above* it, but when the shock is passed *through it*, the phosphoric appearance is very brilliant and has a greenish tint, the light is of short continuance, and the salt is shattered to pieces.

Ammonia. Aqua ammoniæ of the shops is not luminous.

Carbonate of ammonia affords a dense purple spark, radiated on its surface. It is very luminous with a white light, when the shock is taken *above* it; but it possesses a delicate blue or rather purple tint, when the rods rest *upon* its surface, and it is shattered into luminous pieces when the shock is passed *through* it.

Sulphate of ammonia gives a purple hissing stream and is luminous by the shock.

Nitrate of ammonia is not at all luminous when properly prepared and crystallized; but it is slightly phosphoric when prepared with the common aqua fortis of the shops, and not carefully crystallized.

Muriate

Muriate of ammonia affords a purple spark, and is luminous by the explosion.

Succinate of ammonia is luminous when the shock is passed above it, and is readily dispersed in luminous particles when the rods are placed in contact with it.

Acids.

Acids. Sulphuric, nitric, muriatic, phosphoric and acetous acids are not luminous.

Nitric acid affords only a hissing stream instead of a spark. It is extremely phosphorescent with the explosion made *above* it, shining with a greenish light; and when the shock is passed *through* a lump of crystals it is fractured into numerous phosphoric pieces. It is rendered luminous merely by taking sparks from the conductor in its vicinity.

Boracic acid is next in phosphorescency to the citric.

Benzoic acid is almost equally luminous with the boracic.

Tartaric acid affords a hissing stream instead of a spark. It is luminous by the shock, but not quite so phosphoric as the benzoic acid.

Oxalic acid is also luminous by the explosion, but less so than any of the crystallizable acids here mentioned.

Arsenious acid gives no spark, but allows the fluid to pass freely over its surface from the conductor to a considerable distance, giving the sensation of a shock when held in the hand; but when placed upon the conductor and the knob of the discharger rests upon its surface, the spark proceeds from the conductor to the discharger, through the substance of the acid, rendering it semi-transparent. It is very phosphorescent with a white light, when the explosion is made *above* its surface; but when the points of the rods rest *upon* it, the light is yellow tinged with green.

Metallic Salts.

Metallic salts. Nitrate of silver, commonly called lunar caustic, does not give a spark, neither is it luminous by the electric light.

Sulphate of mercury, called turpeth mineral, and white crystallized nitrate of mercury are not luminous.

Howard's fulminating mercury is not phosphorescent by passing the shock above it; but when a grain or two of this salt

salt are placed in the track of the discharge between the points of the rods it explodes with a slight shock. The explosion is accompanied with a dark red or crimson coloured flame and slight detonation. It is a very pleasing experiment, and when exploded upon a plate of glass the mercury is revived, and silvers the plate like a mirror, but it is easily effaced. When exploded upon card, a coloured stain is produced, which is indelible. Metallic salts.

Muriate of mercury, called calomel, is not luminous.

Oxymuriate of mercury, or corrosive sublimate of mercury, affords only a purple stream on its fracture, but the smooth convex surface which has formerly been attached to the vessel it was sublimed in gives a fine purple spark, of a beautiful bright green colour on its surface. It is very phosphorescent by passing the shock above it. Great caution is required in making experiments with this substance, as electricity detaches from it, and throws into the atmosphere, innumerable minute and invisible particles of this caustic poisonous salt, which produce inflammation of the membrane lining the nose, and a very disagreeable sensation in the mouth and fauces, attended with a slight salivation.—Similar effects occur during the explosion of the fulminating mercury, if frequently repeated.

Sulphate, nitrate, and phosphate of copper are not luminous, neither do they give any spark.

Acetite of copper, verdigris of the shops, gives a spark ramified upon its surface, but is not luminous by the electric explosion.

Carbonate or rust of iron gives a spark, but is not luminous.

Sulphate, gallate, and prussiate of iron are not luminous.

Carbonate and acetite of lead give no spark, nor are they luminous.

Muriate of lead is slightly luminous by the explosion.

Sulphate of zinc affords a small purple stream instead of a spark. It is not luminous when the explosion is made *above* its surface, but merely in the track of the fluid when the rods rest upon it, at some distance from each other.

Carbonate of zinc or calamine. Several native specimens

of this substance covering crystals of a calcareous spar, as well as the levigated calamine of the shops, were phosphorescent.

Muriate of antimony is not luminous.

Phosphate of lime and antimony, or James's powder of the apothecaries is very phosphorescent, its light continuing for some minutes.

Tartrite of potash and antimony, or emetic tartar of the apothecaries, is luminous, but not comparable with James's powder.

Miscellaneous Substances.

Compounds containing salts.

Soaps.—Common hard white and brown soaps afford very good sparks, which are sometimes flame-coloured on the surface; but they are not luminous even in the track of the fluid upon their surfaces. Neither is the common soft soap at all luminous.

Sulphuret of potash gives a purple hissing stream, and is not luminous, in which it totally differs from the sulphuret of lime.

Common fulminating powder is not luminous, neither does it explode with the shock which I employ in these experiments.

Gun powder is not rendered luminous by the electric light, nor does explode with a small shock.

In my first letter correct—Vol. XV. p. 281, l. 7 from bottom, after *surface*, insert *and lastly along its surface.*—P. 282, l. 3. from bottom, for *sulphate* r. *sulphuret.*

V.

On the Decomposition of Light into its most simple Elements; a Fragment of a Work on Colours: by C. A. PRIEUR, *formerly Colonel in the Corps of Engineers, and Lecturer in the National Institution.**

White light decomposed into different colours,

WHITE light is decomposed by refraction into an infinite number of parts or rays. They have a different co-

* Abridged from the Annales de Chimie for Sept. 1806, p. 227.

lour

lour at every point in the length of the spectrum, and this colour cannot be varied by a new refraction, if the simplification of the spectrum be at the degree to which Newton carried it. Though in the spectrum thus simplified the lines of demarcation between the colours are by no means very perceptible; it is impossible to ascribe the gradations of their tints to one and the same law. Numerous observations establish the existence of several distinct species of colours; and their division into seven classes, as given by Newton, agrees with a great number of phænomena. Yet some substances, by their peculiar refractive power, derange the spaces of the colours in the spectrum; so that, for example, the green rays are in some instances brought nearer the red, in others nearer the violet. This proves, that the dispersion of the rays does not depend absolutely on their own nature. These are the principal observations it appeared to me necessary to make, in order to shew the present state of our knowledge; and I shall now proceed to examine the action of coloured bodies upon light.

which are really distinct,

but not always refracted in an equal degree, though always in the same order.

I have formerly shewn, that all kinds of transparent bodies, of different colours, which I have observed, transmit ultimately only on the red, or green, or violet rays. The progressive absorption never finishes by any other colours, and I long sought in vain for a substance, in which the final absorption should be of the yellow or blue rays.

The yellow and blue rays disappear first.

Such a result could not fail to excite my attention. I remarked, that, under certain circumstances, the colours exhibited by refraction were almost wholly these three, red, green, and violet: that sometimes yellow appeared to arise from a mixture of red and green, and blue from a mixture of green and violet; which my dial,* as well as the placing of certain coloured glasses on each other, indicated as possible. I perceived too, that the tints of the seven orders of colours might be imitated by the three primitive colours alone which I have mentioned.† This was sufficient to suggest

Sometimes red, green and violet only exhibited by refraction. The others compounded of these.

* This *dial* is simply a circle exhibiting the seven primitive colours conformably to the ideas of Newton. See Optics, Book I. Prob. 2. The author has explained the principles of the construction of this dial, and its leading properties, in his work.

† This proposition of three primitive colours is very different from

suggest to me the idea, that perhaps these three kinds of rays were all that really existed; a proposition that required to be examined with care proportioned to its importance. Accordingly I inquired into the probabilities that might be brought to support it, and compared it with all the phenomena of colour that occurred to me, and lastly I verified it by direct experiments.

The details of these I shall reserve for the last place, beginning with an account of the others.

This hypothesis not inconsistent with known phenomena.

I have already mentioned, that the supposition of three colours was not inconsistent with the formation of all the tints of the spectrum. Neither is it in contradiction with the unchangeableness of each tint by a second refraction: for if a red ray of a certain degree, for example, be found in the spectrum at the same place as a green of a certain degree, their combination will give a yellow of a particular tint; and as these two rays have the same refrangibility, a similar refractive power cannot again separate them. Accordingly, to have a spectrum in all points similar to that which really occurs, nothing more is necessary than to conceive it composed of three spectrums partly overlaying each other; one formed of red rays, differently refrangible, and of different tints; a second, trenching a little upon the first, and having only green rays, but a similar gradation of tints corresponding to their refrangibility; and lastly a third, exhibiting an analogous series of violet rays, and in like manner trenching upon the green. On this hypothesis, there will be no disruption of the whole image, whatever extent be given to it by refraction: besides, it accounts for seven colours separated by lines of demarcation, which no one yet has explained.

Explains the contiguity and distinction of the 7 colours.

To comprehend this, let us look at Fig. 1. Pl. II. which is constructed in the following manner: A right line is divided into seven parts, proportioned to the spaces of the seven colours in the spectrum, and marked by the initials of those colours. On each of the points of division I have

that formerly adopted; for the red, yellow and blue, have hitherto been so considered; while here they are the red, green, and violet, the exclusive existence of which is proved by the analysis of white light in several rays, as will be seen farther on.

erected

erected an ordinate, and afterward drawn the arbitrary inclined line *a d*, then *b g* cutting the former in *c*, and lastly *e h* cutting the preceding in *f*. I suppose, that the modifications of the red rays, on which their different refrangibility depends, are represented by the ordinates corresponding to the line *a d*: these quantities express nothing relative either to the velocity of the rays, or the magnitude of their particles; perhaps they may have a relation to their density, or to any other quality whatever that constitutes their difference. In Newton's system of seven classes of primitive colours, there are likewise red rays differently refrangible; this therefore is not a difficulty peculiar to the state of things I am examining. In like manner the ordinates of the line *g g* will be the modifications of the green: and those of the line *h e* the modifications of the violet. Hence it is evident, that the first division of colours from *a* to *b* will be red alone; that it will be followed by a mixture or combination of green and red from *b* to *c*, in which the quantity of the latter will predominate, and give orange; after which another mixture of red and green will proceed from *c* to *d*, in which the green will predominate more and more, forming yellow; then from *d* to *e* will be green alone; from *e* to *f* the mixture of green and violet that produces blue; from *f* to *g* the mixture producing indigo; and lastly from *g* to *h* pure violet.

Accounts for the brightness of yellow.

But another very striking property of the spectrum, which has not hitherto been explained, is the greater brightness of the yellow compared with the rest. This proceeds evidently in my figure from being the sum of the light of the red and of the green. In the blue too there is an augmentation of light by the union of the green and violet; but the effect is much less than in the preceding instance, both from the nature of those colours, and their extent, though there is some trace of it in the spectrum when properly displayed.

Blue next in degree of light, and why.

By this figure, however, I do not pretend to exhibit any thing more than what may possibly happen. For this reason I have limited the ordinates of each colour by a right line merely; for as the law of their progression is not known so that it is impossible to give the precise curve, I have adopted the simplest line as sufficient for my purpose.

The

Properties of the spectrum delineated on a circle.

The striking agreement of my hypothesis with the peculiarities of the spectrum excited me the more to apply it to the dial of colours. This coloured figure has such singular properties, that the mind cannot easily bend itself to them. How indeed can it conceive the existence of an infinite number of luminous rays, all different yet equally simple? How is it, that taken in pairs from the extremities of every diameter of the dial, that is from any two opposite points, they shall always form the same white? For instance, a certain red ray with a green gives white; an orange with a blue, the same; a violet with a yellow, still the same. What a strange similitude! How again are the seven distinct orders of the spectrum consistent with that insensible gradation of the tints of the dial recommended by Newton, and in fact necessary? Yet all these are so completely supported by experiment, that their reality cannot be questioned.

Solution of the problem.

Thus I had a problem to solve, the complicated data of which seemed at first not to promise a simple solution; yet, after various attempts, I attained my object, as will be seen.

First I considered, that both the nature and quantity of the red, green, and violet rays, which I suppose to be the sole elements of white light, are absolutely unknown. But I could likewise conceive them transformed into coloured matters of such intensity, or condensation, that the mixture of an equal quantity of each should produce exactly white.

In the second place I drew Fig. 2. This consists of three curves nearly circular and alike, described round the dial in the following manner. I first described three equal circles, having their centres in the radii drawn through the divisions of 60, 180, and 300 degrees; and the circumferences of which were tangents to the dial at the divisions of 250, 360 and 120 degrees respectively. I then modified each circumference by this law, that, on prolonging the diameters of the dial in every possible direction, the sum of the prolongations of every diameter to the new curve should be a constant quantity. It is easy to understand this second construction, by which it will appear, that the resulting curve differs in fact little from the circular circumference.

Thirdly,

Thirdly, I conceived, that all the prolongations of the radii of the dial to the red curve represented each a proportional quantity of my red matter mentioned in the paragraph before the preceding; so that this dial is surrounded by a red crescent to a certain point, whence it decreases according to a given law. We must likewise admit a green envelop, analogous to the preceding, and limited by the curve of that colour; and lastly a violet envelop, within the third curve.

This supposed, if for each point of the dial we make a mixture of colours corresponding to that point, we shall have a series of tints in imperceptible gradation from one to the other: which in tone, place, and every other respect, will be extremely analogous to the colours of the dial, that I had previously traced conformably to the ideas of Newton, and are such, that the union of two diametrically opposite to each other, will every where form a white identically the same.

This is a result which I offer as a farther probability greatly in favour of my hypothesis of three colours.

A little difference in the place of the colours.

It is true, the dial constructed by the first method differs a little from that by the last, as in this the purest red is somewhat nearer the place of the orange, and the violet nearer that of the indigo. But, beside that this difference is little in itself, it is supported by experience; for the relation of colours in general, and the progress of their absorption, appear to give some preference to the latter method.

Still I must repeat, that the observations I have here made are only to shew the possibility of the thing; the question can be decided only by the direct examination of the rays of light on the spectrum in its simplest state, and this remains for me to give.

Not easy to obtain a spectrum of the greatest simplicity; and few have done it since Newton.

As few have the means of procuring this very simple spectrum, and there is some difficulty in applying them, I shall enter into this subject somewhat at large. This I conceive to be the more necessary, as few appear to have repeated experiments of this kind since Newton, at least with due precision. Treatises on optics indeed do not mention this repetition formally, many philosophers having attempted

ed it without success, and others having persuaded themselves a little too hastily, that they had completely succeeded; as Abbe Nollet, for instance, whose name has been quoted as an authority.

I should not myself have had the means I long wished, but from the politeness and enlightened assistance of Mr. Trémery. Fortunately his study was provided with every thing necessary; but I shall first briefly describe the nature of the experiment, and the conditions indispensably requisite.

Nature of the experiment.

The business was to repeat the experiment, in which Newton obtained a well defined solar spectrum, the breadth of which, by concentrating the pencil of light, was reduced to $\frac{1}{70}$, or $\frac{1}{72}$ of its length; and which consequently exhibited the homogeneal rays incomparably more distinct from each other than in the common spectrum. Opt. Book I. Part 1. Exp. 11.

Conditions requisite to its success.

I have already hinted above, that the success depends, 1st. in operating on a pencil of light that is very small before it reaches the prism; 2dly. in producing by the prism a considerable dispersion of the coloured rays; and 3dly, in receiving their dissected image on a plane very distant from the point of the angle of dispersion.

Obstacles to it.

But these three conditions are not of themselves sufficient. It is almost impracticable to attain the desired object by their concurrence, when the rays arrive at first in parallel directions; still more if they arrive diverging, as they do when a pencil of light is admitted through a simple hole in the window shutter of a dark room; in which case the sensible diameter of the sun's disk must occasion a divergence of the pencil. There is only one circumstance favourable therefore, that in which the rays may be rendered convergent, without infringing the preceding conditions.

Newton's method, converging the pencil by a lens of little convexity.

The only method of doing this did not escape the sagacity of Newton. He effected it by placing at a considerable distance from the shutter, and but a little before the prism, a lens of a long focus, which by its position regulated the distance of the plane on which the spectrum was to be received. In this way, and by the assistance of some other precautions, he resolved this grand problem in optics.

The

The following is the manner in which we proceeded, and its results.

Difficulty of getting a good lens.

It is not easy to procure a single lens, that shall be capable of giving a focus of ten or eleven feet in the position in which Newton employed it; for several glasses of little curvature, that were lent me as fit for the purpose, were altogether incapable of effecting it. I then imagined, that I might succeed by placing near the shutter an object glass of short focus, to make the pencil very divergent beyond it; and placing at the same time at a sufficient distance, an excellent lens of Mr. Tremery's of five feet focus.

Resource of the author.

His apparatus described.

The effect answered our wishes, and in consequence we arranged our apparatus as follows: 1, on the outside of the windows, a plane metallic speculum, to reflect the solar image: 2, an object glass of 87 centimetres (33 inches) focus, distant from the speculum about 24 centimetres (9, 36 inches): 3, a diaphragm, pierced with a hole six millimetres in diameter (2.36 lines,) and at the distance of 11 centimetres (4. 3 inches) from the object glass, to introduce the pencil of light into the room: 4, a lens of 162 centimetres (5 feet, 3 inches focus, placed 32 centimetres (1 foot) from the object glass: 5, at 11 centimetres (4. 3 inches) from the lens a prism of very clear flint glass, with angles of 60°, covered with black paper on each side, except at the place left for the transmission of the rays; this prism being continued so as to be moveable in different directions, as occasion might require: 6, a board covered with white cloth, at the distance of 422 centimetres (13 feet, 8 inches) from the lens. All these were placed, kept, or brought into the proper directions, suited to their several purposes, and to the course of the sun. The place too was so contrived, as to be rendered pretty dark at pleasure. Having taken every possible care in arranging our apparatus, we were able to obtain every day, when it was fine weather, a very simple spectrum for several hours; which was quite sufficient for our various experiments, at some of which Messrs. Berthollet, the father and son, Mr. Laplace, and other gentlemen were present.

A very distinct spectrum 9¼ inches long.

The spectrum was very distinctly bounded by two rectilinear, and perfectly parallel sides. Its length was a little more

Breadth 1-25th. more than 24 centimetres. (9. 36 inches) Its breadth was $\frac{1}{25}$ of its length, when the aperture in the diaphragm was 6 millimetres. (2. 34 lines) Sometimes this was reduced one half, and the contraction of the spectrum was proportional, the breadth then not being more than $\frac{1}{50}$; and lastly, by diminishing the aperture, it was reduced to $\frac{1}{87}$ of the length.

Colours bright and vivid. At a little distance it appeared triangular, the red forming the point.

As to the strength of the colours, they were vivid and bright as might be expected. The impression on the eye was such, on account of the narrowness of the image, that at a few decimetres (two thirds of a foot) from the cloth the spectrum appeared as two straight lines, forming a small angle, the apex of which was at the red extremity, and the base at the violet. On going nearer it appeared a single line. It was the same, if the spectrum were examined from a distance through a glass. This doubling or radiating of the image depends on the conformation of the eye, and is connected with some other phenomena, of which I may hereafter give an account.

The line of demarcation between the colours not very precise.

The distinction of the colours, and their separation into seven classes, was likewise one of the objects of our enquiry. Though the existence of this distinction was perceived, it must be confessed, that it was not easy to trace all their divisions. I made some attempts to effect it, the narrative of which I shall pass over for the present; merely observing, that Newton did not make his division on a spectrum thus narrowed, but on one much larger, obtained in the usual way without a lens. Opt. I, part 2, prob. I.

More so in the common spectrum.

The green shortened;

the blue and violet lengthened,

from the nature of the glass.

Lastly I shall observe, that the green colour in our spectrum did not extend quite to the middle of its length, whence it followed, that the shades between the green and red were a little shortened, and those of the blue and violet proportionally elongated. These effects were owing no doubt to the nature of the flint glass, of which our prism was made. We had no opportunity of procuring common glass free from streaks. Having tried a hollow prism, formed of glasses joined together, and filled with water; the faces of the glasses occasioned duplications of the spectrum, which rendered it confused; so that we returned to our English flint glass, which, while perfectly void of colour, combined homogeneity of substance, and accuracy

of

of structure, with the finest polish; in short, it was to all appearance free from defect.

Experiments.

It now remains for me to speak of the particular experiments on the analysis of colours, which I had long planned.

Hypothesis, that blue is a compound of green and violet; yellow, of green and red. Consequences.

The reader may recollect, that I had suspected the blue to be merely the result of a combination of green and violet rays; and that in like manner the yellow proceeded only from a mixture of green and red. I reasoned then in this manner: on the supposition that in reality there existed no simple rays of blue, if we prevent the arrival of rays to that part of the spectrum, either by a substance that suffers only the green rays to pass, or by one that allows a passage to the violet only, we shall find beyond these substances only green or violet; otherwise, supposing the blue rays to be simple, they will traverse neither of the substances I have mentioned, as we shall find beyond them nothing but black. We may reason in a similar way with respect to the yellow, which must be subjected to the trial of a red substance and a green.

Requisites for the experiment.

Thus we must be furnished with three substances coloured in the requisite manner. For the violet I employed an ammoniacal solution of copper, in a phial with plane parallel surfaces: for the green a solution of muriat of copper, in a similar phial: and for the red, either wine of a good colour, or a tincture of cochineal. All these must be sufficiently concentrated, or they will transmit other rays, beside those we have in view. This concentration has the inconvenience of rendering the colour obscure, it is true, and this is some obstacle to their use; but it is the only way in which nature permits us to obtain simple colours, and we must be content with it.

Coloured glasses might be substituted for the red and green liquors; but with respect to the violet I could not procure any, on which I could depend.

Every thing being thus prepared, I made my experiments in concert with Mr. Tremery and Mr. Drappier.

Apparatus.

We had a screen, which we could place at will before the cloth opposite the place of the spectrum. In this screen was a small circular hole 3 or 4 millimetres (I. 17 or I. 56 line)

line) in diameter, by means of which we could allow a small coloured pencil to fall on the cloth, while all the rest was dark. We could easily ascertain, that this little pencil, taken successively from the different colours, was simplified as much as possible by refraction; for the circular spot, examined some distance with a prism, was not at all irregular.

We then passed through the small hole a pencil of very decided blue; and in this respect our latitude of choice was great, since in our spectrum the blue had an extent of more than 54 millimetres. (2 inches) The blue spot being well formed on the cloth, the green phial was placed before the hole; when the light of the spot was immediately much weakened, and its colour changed to green. On substituting the violet phial in the place of the green, the spot became violet. This experiment was repeated several times, that we might convince ourselves of the fact, and succeeded uniformly.

Blue light transmitted through a green medium was green; through violet, was violet.

The trial with the yellow succeeded in like manner; it was changed successively to red and green, according to the substance opposed to the rays.

Yellow changed to red and green.

Another day these experiments were repeated with some little alterations. When the small round image fell upon the cloth, we went behind to look at it; and found that it passed through, appearing on the back of the cloth, which was muslin well stiffened with starch. In this way we could make our experiments more conveniently, as we had only to cover our eye with a coloured substance, and look at the little spot through it. When the spot was formed by blue light, it appeared green, or violet, according to the substance interposed. Through a red substance no light was seen: a proof, that the preceding effect did not arise from white light mixed with blue. If it were viewed through an orange glass, the property of which is to absorb only the blue and violet rays, the spot appeared green; a proof that it was formed in reality by green and violet rays.

Experiment varied by viewing the image from behind the screen through a coloured substance put before the eye.

Blue light, as before.

Finally the yellow spot exhibited similar appearances; altogether invisible through a violet substance, it shewed itself green or red through substances of these colours.

Yellow, the same

Such are the results, that confirm my opinion of the elementary parts of light. Now let these be combined with the

Arguments for three primitive colours, red green, and violet.

the effects of absorption, which ultimately leaves only red, green, or violet rays; with the simple and natural explanation of the principal appearances of the spectrum, by means of three kinds of rays; with the happy manner in which these three kinds are applicable to the properties of the dial of colours, and remove its complication; and I think the whole will support my proposition. If it do not hence appear to the natural philosopher as a fact established beyond all question, at least he cannot refuse to consider it as already grounded on strong probabilities, and sufficiently interesting to merit a thorough investigation, which my occupations have prevented me from pursuing any farther.

Recapitulation.

Red and green produce yellow; green and violet, blue. Intermediate shades from different proportions.

Thus our system of colours appears to me reduced to these few data: three sorts of luminous rays, of a particular and unknown nature; red, green, and violet. Combined by twos, the red and green produce yellow; the green and violet, blue; the violet and red, purple: the three together produce white; and lastly, the intermediate shades are according to the proportional quantities of their elements.

Light analysed by refraction.

Bodies exercise a general action on all the rays of light, and a particular one relative to their peculiar nature. If the white pencil fall obliquely on the surface of a transparent body, the rays, as they penetrate it, deviate from their original direction, some more, others less, according to their nature. Here we have a true analysis of white light, in which its three simple elements may be found separate, as well as combined, in different proportions. It is thus that refraction exhibits a series of tints, which differ, in different bodies, both with respect to their general deflection, measured by its mean quantity, in the relative dispersion of the rays, and in the particular position of each colour.

Bodies coloured from absorbing particular rays.

If the affinity of the body for the rays of light, be such as to absorb some into its own substance, it will be coloured; and will exert a preferable or stronger action on certain sorts of rays. In a small mass, the body will first absorb these rays, to which it has a preferable affinity; and, if its action

action on the two simple kinds do not give a marked preponderance to one of them, it will be a mixed combination, that will first disappear. The mass of the body being gradually increased, the destruction of the rays will go by new mixtures, still progressively; the kind least acted upon will remain the last, and it will necessarily be one of these three, red, green, or violet; after which no more light will be transmitted. Such are the phenomena of absorption, and its different gradations.

Absorb others in succession, till by increasing their thickness they transmit none.

I shall give here one of the last results of my experiments, which might have created some confusion, had it been mixed with the preceding considerations, yet tends to confirm their principles.

White produced by combining opposite points of the spectrum.

I had an inclination to try, whether the light from a given part of the single spectrum, combined with that of another part chosen for the purpose, would produce white.

To carry this into execution, I placed before the image of the spectrum, received on the cloth a screen, by which one portion was concealed, and another left open. This screen, however, was perforated by a small hole, through which passed a ray of coloured light belonging to that part of the spectrum which was concealed. Lastly, this little coloured ray was received on a metallic speculum, placed between the screen and the cloth, and inclined so as to throw it on a given point of that part of the spectrum, which arrived freely on the cloth. Thus the colour arising from the mixture of two rays of light was observed.

I varied the trial of this apparatus on different points, the corresponding tints of which were diametrically opposite on the dial of colours. In several instances I did not obtain a white free from all tint of colour, because, a certain proportion in the quantity as well as quality of the elements is necessary: but having carried the little image almost to the limit of the green and blue, it gave a decided and bright white, when thrown on the extremity of the red.

Clearest white from the extremity of the red, and boundary of green and blue.

This very remarkable fact adds fresh support to my proposition respecting the compound state of the colour in certain parts of the spectrum, simplified to the utmost.

For,

Decomposition of Light.

Fig. 1.

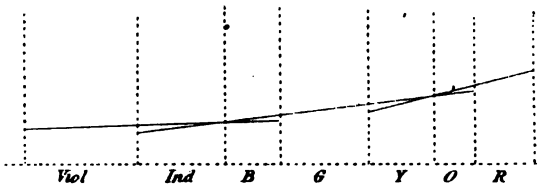
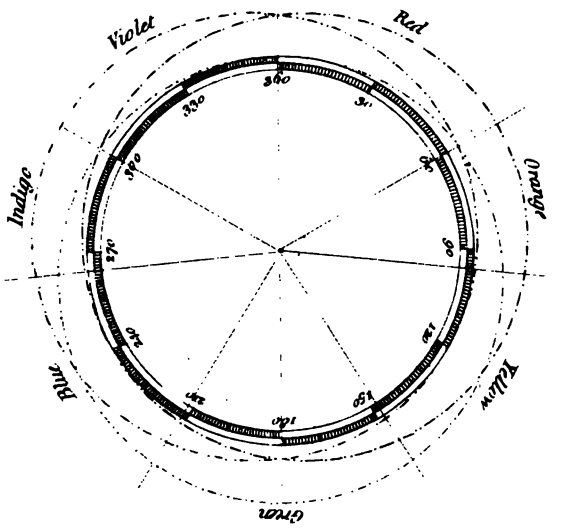


Fig. 2.



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For, if the combination of the three colours I have mentioned be necessary to produce white, as every thing tends to persuade us, we must admit the existence of violet in the greenish blue with which the experiment was made.

N. B. It may be necessary to add, in explanation of Fig. 2. Plate II. that the colours of the three circles are distinguished by dotted lines; the red by round and long points alternately; the green by one long point and two round; the violet by one long and three round.

VI.

Wooden Matches for Artillery to be used instead of Rope Match, or Port-Fires: read at the National Institute, April 1806. *By* C. L. CADET*.

FOR several centuries rope match only was used for firing great guns, mortars, howitzers, and other pieces of artillery. This match, as is well known, is a rope of supple hemp, of a midling size, boiled for two hours in a bath of saltpetre, ashes, quicklime, and horse-dung. This rope, when dried, burns slowly to the end, in the manner of touchwood, and communicates its fire like red-hot coal. For use it is twisted round a staff called a port-match, and left to project near five inches beyond its end, this length burning an hour. Rope or common match.

This match has several inconveniences. It requires constant attendance, since it must be unrolled from the staff every hour, or oftener; a tolerably heavy rain puts it out; it gives the artilleryman no light by night; and the end beyond the staff is not always steady, so that the gunner is slow in firing his piece. In these accounts its use is now confined to garrisons, except for carrying fire in the field, where for other purposes port-fires are employed. The inconveniences.

These port-fires are paper tubes, filled with a mixture of sulphur, saltpetre, and a very little neat-powder. This composition, the greater part of which is saltpetre, burns and melts with great activity, giving a vivid and bright Port-fires.

* *Annales de Chimie*, Sep. 1806, p. 314.

flame,

Their advantages.

flame, which quickly sets fire to the priming. In this respect they are far preferable to match, since they give light to the gunner, their fire is more vivid, and they are more easily guided; but these advantages are counterbalanced by dangers and defects. The saltpetre in these port-fires is never entirely burnt, but part runs out of the tube. When the materials are not well powdered, they are subject to spit, or throw out pieces of burning saltpetre to the distance of three or four feet, which may occasion serious accidents, particularly on board ships. I myself had my hair set on fire, and a hole burnt through both my coats, by a spark of this kind. In ships they are obliged to be kept in the middle of a tub of water on this account.

Defects.

Particularly dangerous at sea.

Wood impregnated with nitrate of copper, proposed by Borda and Proust.

These were the only means employed to fire pieces of artillery, when one of my correspondents at Madrid acquainted me, that Messrs. Borda and Proust had proposed to the Spanish government, to substitute instead of the cannon match, wooden rods impregnated with nitrate of copper. He added, that these rods burnt like touchwood, forming a pointed red coal; and that the trials with them succeeded perfectly, though they had not been adopted. I informed his excellency, the minister at war, of this new method; and he requested me to make the necessary experiments for ascertaining its utility, directing Mr. Lespagnol, a captain in the artillery, to assist me in the inquiry.

All wood not equally good for the purpose.

My first idea was, that all kinds of wood could not be equally fit for the purpose; and that the difference of their porosity would occasion a difference in their combustibility.

Nitrate of potash will not do.

Before I tried the metallic nitrats, I took common saltpetre, and boiled several kinds of wood in a strong solution of it, which they imbibed in different proportions. This attempt did not succed: the only wood that burnt quickly was the common cane, used for dusting clothes, or *rotang;* but its coal had no substance, the least blow breaking it off, and extinguishing it. I then got a joiner to make me some square rods, half a yard long, of oak, elm, ash, elder, birch, poplar, lime, and fir. I took two parcels of these, and boiled one in a solution of

Different woods boiled with nitrates of copper and lead.

of nitrate of copper, the other in a solution of nitrate of lead. In each, the oak, elm, ash, and elder, were not saturated, and burnt in the usual manner: the others afforded me very good matches. But before I enter at large on their properties, I shall observe, that I conceive the nitrate of copper should be rejected, because it is too dear, it quickly corrodes the boilers, and its vapour is noxious. Accordingly I confined myself to the nitrate of lead; and I found after several trials, that it answered the purpose completely.

Oak, ash, elm, and elder, do not answer.

Birch, poplar, lime, and fir, do.

Nitrate of copper disadvantageous.

The wood that did best was that of the lime, birch, or poplar. To compare their properties, I weighed some rods both before and after boiling; I ascertained how much their weight was increased, and how long they continued burning; and I calculated how much of each a pound of nitrate of lead would saturate. The following table gives the proportions.

Lime, birch, and poplar best.

Their properties compared.

Name of the wood	Weight of a yard before the experiment. Grains.	Weight after. Grains.	Gained in weight. Grains.
Birch	888	1416	528
Poplar	516	936	420
Lime	888	1728	840

Name of the wood.	Length saturated by a pound of nitrate of lead. Yards.	Ft.	Inches.	Time each continued burning. Hours.
Birch	17	1		3
Poplar	21	2	8	2
Lime	10	2	9	3

From this comparative trial it follows, that the lime tree affords the best wood for matches for artillery; and with it I made the experiments desired by the minister, in presence of Mr. Lespagnol.

There are circumstances in which the service of the artillery requires light. Rods impregnated merely with nitrate of lead, produce a coal sufficient to discharge a cannon, but no light is afforded by them I conceived, that, if they were impregnated with oil of turpentine, they might

Light sometimes necessary.

Subsequent impregnation with oil of turpentine produced this.

might yield flame, without detriment to the action of the nitrate; and my hopes were realised, for rods thus prepared furnished both light and fire at pleasure. In this addition I found two other advantages: one, that of rendering the wooden match impervious to water; the other, that of facilitating the reduction of the lead, part of which I was apprehensive might be carried off in vapour, and injure the health of those who respired it.

Two other advantages in this.

The author's theory.

The theory of the process I adopted is simple; and it is easy to explain, why metallic nitrates succeed better than nitrate of potash. However dry the wood may be, it always retains a little of its water of vegetation or of composition, which is an obstacle to its proper combustion. By boiling the rods in a solution of nitrate of lead or of copper, which on account of its specific gravity requires a high temperature; this fluid dilates, softens, and penetrates the fibres of the wood, and expels their water of vegetation, which is replaced by that of crystallization. The nitrate then comes into immediate contact with the carbon of the wood, whence the rapidity of its combustion. The nitrate of potash does not answer so well, because, retaining much water of crystallization, its solution does not acquire so high a temperature: and, supposing it able to penetrate the wood as intimately, it carries into it too much water, for its combustion to be progressive and continual. A proof of this reasoning may be found in the composition of the two salts: nitrate of lead contains ·75 of its base, that of potash but ·49.

Lead easily reduced.

Its acetate converts rope into a match.

The rapid combustion of the wooden match is owing also to the facility, with which the salts of lead are reduced, when in contact with burning charcoal. If a hempen rope be boiled in a solution of acetate of lead, and afterward dried, it may be used as a match. It burns slowly like touchwood, and has a very bright coal. The oxide of lead, as the metal is reduced, gives out its oxigen to the carbon, and accelerates the combustion *.

Light woods absorb most salt.

On comparing the specific gravity of wood with its satu-

* We have a familiar instance of this in the popular experiment of burning a red wafer in the flame of a candle. Ed.

ration

ration by salts, we find, that the lighter the wood, the more saline matter it absorbs into its pores, or the interstices of its fibres. Hence it appears to me we may infer, that it contains less carbon than a heavier wood in a given bulk; and that its combustion will evolve less caloric, since the caloric emitted is in the ratio of the quantity of oxigen combined with the combustible. It seems to me, that we might class different kinds of wood, as to their combustibility, by their absorption of salts; and thus find which would be most advantageous to burn for domestic purposes, whether we would have a rapid combustion, or a stronger and more continued heat. These researches will form the subject of a particular work, which I purpose on all our forest trees.

This absorption a test of their combustibility.

The wooden matches, compared with port-fires, have the following advantages.

Comparison between the wooden match and port-fires.

The port-fire lasts but three or four minutes.

A match a yard long will burn three hours.

The port-fire is liable to break in the boxes.

The match is strong, and easily carried about.

The port-fire throws out dangerous sparks:

The match confines its fire to itself.

The port-fire costs from three pence to four pence half-penny:

The match costs but three half-pence or two-pence.

The last consideration is of great importance, since, from calculations made in the war-office, what would cost the state in the one case a thousand pounds, in the other would not come to more than seventy-five*.

Great saving.

As it was necessary to ascertain, whether these new matches would resist the rain, I had several burnt during long and heavy rains, and they were not extinguished till they were totally consumed; their combustion being a little retarded only.

The wood matches not extinguished by rain.

As the fabrication of these matches requires some care and precaution, I shall conclude this paper with a minute description of the process, agreeably to the request of

Precautions necessary in making them.

* According to the estimates just before given, the saving would be much greater than this on the lowest calculation. T.

his excellency the minister at war, for the instruction of the artificers employed in our arsenals.

Method of preparing the combustible wooden Matches for Artillery.—Shape of the Matches and Choice of Wood.

Shape of the match and choice of the wood.

The matches should be parallelipedons, half a yard long, and half an inch square. The best wood for them is that of the lime tree, or birch; but for want of these, poplar or fir may be used. Any white and soft wood might be taken, if necessary; but those above-mentioned are to be preferred.

Round inferior to square.

The shape might be supposed of no consequence: yet experience proves, that round matches do not furnish so good a fire as the square. The angles of the latter keep the coal in the centre burning vividly, and the match always terminates in a burning cone two inches long.

Drying the Wood.

The wood must be thoroughly dry.

Before the matches are saturated with nitrate of lead, the wood must be perfectly dry. For this purpose the wood should have been cut and stored at least a twelvemonth; and the matches, after they are shaped, be exposed for half a day to the heat of a stove at 30° (by what thermometer is not mentioned; probably 90°, or perhaps 100° Fh.) For want of a stove they may be put into a baker's oven, when the bread is drawn.

Furnaces and Boilers.

Boilers.

The fabrication of the matches requires two furnaces and two boilers. The shape of the boilers should be that of a fish-kettle, narrow, and three quarters of a yard long. Their size should be proportional to the quantity to be made at a time.

Furnaces.

The furnaces should be constructed so that the heat may act uniformly on every part of the bottom of the boiler.

First boiler.

The first boiler must be of copper, well tinned, and provided with a plate of the same metal, to press down the matches, and keep them immersed in the boiling solution.

Second Boiler.

The second boiler may be either of copper or of cast iron, placed on a sand bath, and having no

no direct communication with the fire. It should have a lid fitted to it very closely; and handles to lift it up when necessary.

Preparation of the Nitrate of Lead.

Preparation of the nitrate of lead.

To make this salt, nitric acid, or aqua fortis, must be saturated with red oxide of lead, or with litharge: but as it is necessary that the salt should be neutral, and have no excess either of acid or of base, some precautions in this operation are necessary. If the acid be too much concentrated, the salt will unite in a mass, crystallize confusedly, and contain a great deal of uncombined oxide. If too little oxide be used, the salt will be acidulous, and soon destroy the boilers. To obtain the mean term, 500 parts of litharge should be put into a vessel of glass or earthen ware, and on this should be poured 416 parts of nitric acid at 40°, (specific gravity we believe, 1.386) diluted with 128 parts of water; heat the mixture till the oxide is dissolved, filter, and evaporate to dryness. These proportions ought to produce 640 parts of nitrate of lead.

Proportions of the ingredients.

Bath of Nitrate of Lead.

Liquor for the boiling.

The nitrate of lead is very soluble in water, and the least possible quantity of liquid should be employed, that the bath, fully loaded, may acquire a temperature far beyond that of boiling water, and thus insinuate itself easily into the pores of the dilated wood. Accordingly, for every pound of nitrate, only a wine quart of water should be put into the boiler, or thereabout: but as different kinds of wood do not saturate themselves equally with the salt, their proportions must be studied. Experiment has shown, that to absorb a pound of nitrate of lead, requires near eleven yards of lime wood, 17½ of birch, and near 22 of poplar. The lime therefore, when saturated, is the most combustible.

Proportions of nitrate to different woods.

To render the saturation of the wood complete, six hours boiling are necessary, and hot water must be added, when the bath sinks so low as to let the salt fall to the bottom.

Time of boiling.

Second drying of the Matches.

When the matches are taken out of the boiler, they must be

Second drying.

be carried to the stove, and made thoroughly dry, before they are put into the following bath.

Turpentine Bath.

Boiling in oil of turpentine.

Into the second boiler is to be put as much oil of turpentine, as will cover the matches to the depth of about an inch; and this is to be heated gently, till it begins to boil. But the moment it grows white and rises, the boiler must be covered, and quickly lifted off the sand bath, lest the oil should take fire. This boiling should be repeated two or three times, which will take about half an hour: the bath then is to be left to cool; the matches are to be taken out and wiped; and lastly they are to be dried in the stove, when they will be ready for use.

This paper was approved by the Institute, at its meeting on the 5th of May, on the report of Messrs. Carnot, Deyeux, and Guyton de Morveau.

VII.

Letter from a Correspondent on the Means of destroying the Insects which infest the Houses in large Towns.

To Mr. NICHOLSON,

SIR,

Introduction.

AS you do not think it beneath the dignity of your Journal to descend to the disgusting, although often necesary business of considering the best method of destroying bugs and fleas, the following observations, suggested by your correspondent A in the last number of your Journal, are at your service, if you think they are worth insertion. I shall be very glad if they contribute to relieve your correspondent or any other of your readers from one of the "miseries of human life."

I am, Sir,

Yours,

W. W.

St. Mary-le-bone, May 6th, 1807.

Domestic insects infest dwellings in

Bugs are often intolerable pests in houses in large towns; more especially in inns, hotels, lodging-houses, &c. which are

are exposed to continual importation of them upon clothes, packages, &c. When once they get into a house, although the numbers may be kept under by cleanliness, frequently taking down the bedsteads, and washing them with various kinds of poisonous washes; it is generally found that they cannot be eradicated. Their eggs or knits, or at least some of them resist the action of the poison, and after a time fresh swarms are produced, who live and multiply, especially in hot weather, in the apparently poisoned wood. Soon too they get into the wainscoat, skirting boards, or lath and plaster walls of the room, from whence they send forth fresh colonies as the former are destroyed.

instances where care and cleanliness are not sufficient remedies.

Six years ago last September I took my present dwelling house. The walls were repaired and white-washed. The bedsteads, one excepted, were all new, and that one was perfectly clean. Precautions were taken to prevent bugs being introduced in any old boxes, &c. of the servant.

Account of a house so infested.

Early in the spring all the bedsteads were, to my vexation and surprize, overrun with bugs—one in particular must have afforded habitation to several thousands.

They were all taken down and washed in soap and water, the ends were dipped in boiling water and then in a hot decoction of the cucumis colocynthin of Linnæn, or bitter apple as it is commonly called. In about six weeks it was necessary to take them down again. They were now washed in essential oil of turpentine, which kills this insect almost instantly; but appears to have no effect on its nit, and wholly evaporates in a few days. Therefore the joints were well brushed with a strong solution of oxymuriate of quicksilver, with which I hoped to render the wood poisonous and uninhabitable to them. But I soon found my hopes were vain.

Application of the usual remedies.

About this time I learned from a neighbour, that during the time of a gentleman who had lived in the house above twenty years, it had become overrun with this insect to a degree that appeared incredible? and that, until his death, he would not suffer his bedstead to be touched. The insects were sometimes seen crawling even upon the walls of his drawing room. After his decease millions were found upon his bed and chamber furniture.

I need

Remedies against the insects which infest our houses.

I need hardly say that this intelligence and my expence made me wish myself out of the house. But as I could not conveniently put my wish in execution, it was necessary to try to get rid of my co-tenants.

The bedsteads were taken down every three or four weeks during the summer and washed with decoction of hellibore, solution of arsenic, and various other poisonous washes. Generally some living bugs were found in them.

Early the next spring they were again taken down, and we had the mortification to find fresh colonies had taken possession, and were beginning to breed in the joints.

The skirting boards of the bed rooms were now removed, and in such rooms as were papered, all the loose paper was removed, and the rooms were well fumigated with oxymuriatic gas; after which the walls that were papered, were covered with paint; for fresh papering walls infested with this insect favors their increase.

The joints of the bedsteads were painted over with three coats of oxyde of lead mixed with linseed oil and a little rosin, so as to form a thick coat over the wood.

From this time, excepting a few stragglers who had got into the joint and died there; the bedstead continued quite free from the insect for two years, when a few were found in some parts where the paint had been abraded. The joints have since been painted over with a coat of thin paint once in two years. A precaution which I have used because the walls are not free from them. During very warm weather one or two are sometimes found upon the furniture; but such as get into the joints die.

Another bedstead, the joints of which happened to be painted with Spanish brown; bugs were found in the following year.

I wish this method may be found generally successful. The joints should be made easy and free from splinters, also all cracks and useless holes should be immediately filled up. If the insect has got into the walls the beds should stand six or seven inches from them.

On the subject of fleas I have no experience. Where from particular local situations cleanliness alone is insufficient to keep them away, the blankets may be dipped, and the floor,

floor washed in a decoction of the cucumis colocynthin, which I am told is poisonous to them. Your correspondent, if he pleases, may rub his body with it—it is perfectly safe; or he may put into his bed a bunch of fresh rue or savin, or perhaps of any other strong smelling herb.

VIII.

Theory of the Fabrication of Sulphuric Acid; read in the Class of Physical and Mathematical Sciences of the French National Institute, January the 20th, 1806, by Messrs. Desormes *and* Clement.*

DIFFERENT opinions are entertained respecting the utility of nitrate of potash in the usual mode of fabricating sulphuric acid. Some believe, that the high temperature produced by its deflagration determines the formation of sulphuric acid; others imagine, that the nitrate affords the quantity of oxigen necessary to complete the combustion, which the atmospheric air has commenced: others again suppose, that water may be decomposed in the process, &c. We shall here attempt only the refutation of the first and second of these hypotheses, which appear at first sight the most probable.

Nitrate supposed to increase the temperature, afford oxigen, &c.

The first cannot be maintained, because, at the same time as nitrate of potash is added to the sulphur, clay and water are frequently mixed with it, each of which has the effect of diminishing the temperature; one by rendering the combustion more slow; the other by constantly absorbing a large quantity of the caloric evolved, to acquire the state of vapour. Besides, it is known, that sulphur burnt by itself, at a temperature of 1000° of the centigrade thermometer for instance (1832° F.) affords no trace of sulphuric acid.

Temperature not increased, because clay and water are present.

The other hypothesis, which does not appear so remote from the truth, is notwithstanding equally erroneous. It admits, that the oxigen extricated from the nitrate of potash is sufficient for the conversion of all the sulphureous acid

Oxigen of the nitre too little to convert the sulphurous acid into sulphuric.

* An. de Chim. Vol. LIX. p. 329, Sept. 1806.

acid gas produced into sulphuric acid; but the contrary to this is easily proved. The quantities of the elements that concur in this operation, or result from it, are not known with precision; yet those we shall assume may be considered as sufficiently near the truth to refute the second hypothesis.

Shewn by the proportions of their principles.

Nitrate of potash contains about 0.30 of nitric acid; which acid, according to Davy, contains 0.70 of oxigen. In this nitrate therefore there are $0.70 \div 0.30 = 0.21$ of oxigen. In sulphurous acid there are about 0.59 of sulphur, and 0.41 of oxigen; and in sulphuric acid 0.52 of sulphur, and 0.48 of oxigen. Now if we employ a very large receiver, or long continuance in a small one into which the air can enter, all the sulphur burnt with $\frac{1}{9}$ of its weight of nitrate of potash will be converted into sulphuric acid. Thus, if we operate with 90 parts of sulphur and 10 of nitrate of potash, we shall have $\frac{90 \times 100}{59} = 152$ of sulphurous acid, which will produce $\frac{90 \times 100}{52} = 173$ of sulphuric acid, and consequently require $173 - 152 = 21$ of oxigen. But the 10 parts of nitrate of potash, employed in this operation, could not give more than 2.1 of oxigen, or a tenth of the quantity necessary to saturate the acid. Some manufacturers carry the proportion of nitrate of potash to the sulphur as far as 0.2; but in this case, which is the most favourable to the hypothesis we controvert, the nitrate is but $\frac{1}{15}$ of what would suffice according to the proportions admitted. The nitre therefore does not serve to produce sulphuric acid, as has been supposed. If its oxigen be not sufficient to convert the sulphurous acid into sulphuric, still less can it suffice to saturate the sulphur with oxigen, without the assistance of the atmospheric air; and it is remarkable, that the acid contained in the sulphate of potash, the residuum of the combustion, contains more oxigen than the nitrate could furnish.

If any doubt of the solidity of this reasoning remain, on account of the uncertainty of the proportions of the substances operating, they will soon be dissipated, when the perspicuousness of the new theory is contrasted with these vague opinions.

When

When we attentively observe the burning of the ordinary mixture of sulphur, nitrate of potash, and wet clay, we perceive, that the nitric acid is not completely decomposed, and that a great deal of nitrous acid gas passes into the leaden chamber with the sulphurous acid. Its colour renders it very visible, and it is a fact that cannot be questioned.

In the ordinary process nitric oxide evolved.

This observation affords a key to the true theory; and in following up its consequences we find the production of sulphuric acid clearly explained.

This the key to the true theory.

We are certain, that the combustion extricates a mixture of nitrous acid gas, and sulphurous acid, with water in vapour, and nitrogen gas from the atmospheric air. We may suppose too, that a portion of oxigen has escaped the action of the sulphur. This supposition, which has nothing in it that is not extremely probable, is the only thing on which any doubt can be entertained. Now, from an experiment made purposely to ascertain this, the sulphurous acid gas and nitrous acid gas cannot exist in contact, without the latter being decomposed, and converting the former into sulphuric acid; this then will take place, when such a mixture of the two gases takes place in the leaden chamber. Being then at a distance from the place of the combustion, this mixture finds a lower temperature, which occasions the condensation of part of the vapour; the rain thus formed carries with it the sulphuric acid produced, and affords a vacuum to the different substances that remain; these precipitate themselves into it in eddies, and present to each other a thousand points of contact that favour the action of their affinities.

Recital of the facts.

Nitric oxide converts sulphurous acid into sulphuric.

After the first production of sulphuric acid, there remain nitrous oxide gas, sulphurous acid, atmospheric air deprived of part of its oxigen. The nitrous oxide necessarily converts itself into nitrous acid, which will be again decomposed to the profit of a second portion of sulphurous acid; and this will go on till all the nitrous acid or atmospheric oxigen, or both, are exhausted.

The nitrous oxide remaining takes oxigen from the air, becomes nitric oxide, and then yields its oxigen to the sulphurous acid.

The first productions of sulphuric acid must be the most copious and rapid, because the condensation of the aqueous vapour produces a great commotion in the mixture of the different gases; and because too the abundance of the oxigen

The sulphuric acid produced most copiously at first.

igen and sulphurous acid render the contact more probable, while, as they become less in quantity, the nitrogen, which continues the same, renders their approximation more difficult.

Residuum, nitrogen, nitrate or nitric oxide, and perhaps oxigen.

After the whole of the sulphurous acid is converted into sulphuric, the substances that remain are a great deal of nitrogen, nitrous oxide, or nitrous acid gas, if there were at first more oxigen than the sulphurous acid required; and perhaps an excess of oxigen more than sufficient to saturate the sulphurous and nitrous acids.

Quantity of oxide of nitrogen remaining.

What is of importance to be observed is the base of the nitric acid, the quantity of which cannot have varied, and which ought to be as much after the production of all the sulphuric acid as at its extrication from the nitrate of potash. This quantity of nitrous oxide, or nitrous acid, is probably a little less than the nitrate could have produced, because in the combustion the temperature may have been raised too high, and then the complete decomposition of a small portion of nitric acid takes place. We say a small portion, because experience has shewn the advantage of keeping the temperature very low by a suitable quantity of moisture.

The nitric acid acts as an intermedium.

Thus the nitric acid is only the instrument of the complete oxigenation of the sulphur; it is its base, the nitrous acid, that takes oxigen from the atmospheric air, to present it to the sulphuric acid in a state suitable to it.

Water indirectly necessary.

We see that water is not directly necessary to the production of sulphuric acid; its combination with what is formed merely effects the extrication of the nitrous acids that must have combined with it. This gas, thus set free, proceeds afresh to seek oxigen from the atmospheric air contained in the receiver, to unite it again with the sulphurous acid. The aqueous vapour has at the same time the double advantage of producing a great commotion in the remaining gases, and of producing this evolution of nitrous acid gas; accordingly its utility has been perceived, and a quantity is introduced, by the exhalations from the hearth, beside that arising from the humidity of the mixture.

Thus setting out from the existence of nitrous acid and sulphurous acid gases, we have followed the metamorphoses these two bodies undergo, taking for our ground-work

facts

facts well ascertained; and have admitted only one single supposition, that of the existence of a portion of oxigen still free after the passage of the air over the sulphur. If this supposition should appear doubtful, at least it will cease to be so, when we have shewn by experiment, that, admitting it, every thing takes place as we had conjectured.

The whole process may be seen in a glass vessel.

By mixing in a transparent vessel the different substances we have considered as essential to the operation, we can see whether the succession of combinations be such as we had conceived. And it may be verified by putting into a glass body sulphurous acid gas, atmospheric air, and nitrous oxide gas in small quantity, for instance $\frac{1}{10}$ the weight of the sulphureous acid; for we see the oxide grow red, and diffuse itself throughout the whole space; then clouds of white fumes roll across the vessel, and deposit themselves in shining stellated crystals against its sides. These dense whirls of sulphuric are succeeded by an appearance of clearness; and, if at this instant a little water be admitted, the crystals of acid dissolve with great heat; the nitrous oxide gas, again becoming free, changes afresh to a red vapour; and the same phenomena re-commence, till all the atmospheric oxigen is consumed, or all the sulphurous acid burnt.

The remaining gases are precisely those we mentioned in our conjectures; for the colour of the nitrous acid appears with almost all its first intensity; and after the operation is completed, there is no more smell of sulphurous acid, but a great deal of nitrogen, and of oily sulphurous acid on the sides of the glass.

Too much water would produce nitric acid.

If in this combustion of the sulphurous acid there were too much contact between the gases and the water added, either by great agitation of a little, or by the presence of a large quantity, the operation would be very slow and incomplete, because liquid nitric acid would be formed, which retaining its state, would have very little action on the gas to be organized. *

The experiment does not always succeed.

* It sometimes happens, that the decomposition of the nitrous acid gas is carried so far as to the state of an oxidule of nitrogen; this too appears to arise from too great action of the water on this gas. Messrs. Berthollet and Guyton have ascribed to this the miscarriage of the experiment, when the contact of water is too great.

This

This experiment, the only one of the kind, leaves no doubt respecting the theory of the fabrication of sulphuric acid, which we have here offered, and which is only a simple exhibition of the facts. If the chain of ideas to be adopted, in order to arrive at the process actually pursued, and the few analogies this operation has to all that we know, be considered, it will appear very fortunate, that chance alone, in some sort led to the discovery; and that we were then put in possession, without knowing it, of the only process perhaps capable of furnishing sulphuric acid by the combustion of sulphur in the air.

The discovery of the process owing to chance.

This theory, affording us the means of improving our knowledge of the proportion of the elements of sulphurous and sulphuric acid, gives us some hope of discovering the same mode of action in other chemical operations, perhaps ill understood; it likewise permits us to add some improvements to the present mode from just principles; as the extent and form of the leaden chambers, and the management of the fire, must be necessarily influenced by this hypothesis; but its first benefit will be a saving of almost the whole of the nitrate of potash.

Advantages to be expected from this theory.

P. S. In the meeting of the 1st. of September, 1806, the Physical and Mathematical Class of the National Institute ordered this paper to be printed in the collection of those of learned contributors.

IX.

Facts toward a History of Cobalt and of Nickel, by Mr. PROUST; *abridged by Mr.* CHEVREUIL.*

Action of acids on cobalt.

SULPHURIC, muriatic, and nitric acids, oxide cobalt in the same manner. With the first and second hidrogen is evolved.

Sulphates.

Two sulphates.

Of these there are two, one simple, the other a triple salt, with the addition of potash or ammonia.

* Annales de Chimie, Vol. LX. p. 260, December, 1806.

1. The

1. The simple sulphate has a taste slightly pungent, and a little bitter, with something metallic. Its crystals, which are of no great bulk, are sections of irregular octaedra heaped together, of a gooseberry red colour, and unalterable in the air. By distillation they lose 42 *per cent.* of water, and are rendered rose-coloured and opake. In this state they can endure a red heat without being decomposed, except in the points that touch the retort. Simple sulphate.

2. When sulphate of potash is mixed with the preceding sulphate, we obtain more bulky crystals, which are rhomboidal cubes. This triple salt is less soluble than the simple sulphate, and loses only 26 *per cent.* of water by distillation. Triple sulphate.

Carbonate.

Carbonate of potash produces 40 or 42 hundredth parts of carbonate of cobalt with the simple sulphate. An excess of alkali dissolves a great part of the precipitate. Boiling, or cold water, decomposes this solution. Carbonate.

Oxide at a Minimum.

A hundred parts of the carbonate, after the separation of the water and carbonic acid, leave 60 or 62 of greenish grey oxide. To have it very pure, the retort must be as full as possible, and heated gradually. Without these precautions it will be mixed with oxide at a *maximum*, which yields oxigen gas with muriatic acid, while that which is pure does not yield an atom. Greenish grey oxide.

The grey oxide dissolves with heat in nitric acid, without yielding nitrous gas. Heated in contact with air it immediately becomes black; an oxide of which part is carried to the maximum is easily detected by the application of a weak acid, which dissolves only the oxide at a minimum. Ammonia produces the same separation, as Thenard observed. Heated in the air becomes black, and insoluble.

Oxide by Precipitation.

1. A few drops of nitrate of cobalt dropped into *boiling water* alkalized with potash give a blue precipitate, which ultimately becomes of a rose colour, if the boiling be continued. In this case a hidrat is formed. Nitrate precipitated by potash and converted into a hidrat by boiling.

2. If

Without heat the precipitate is green.

2. If *cold alkalized water* be employed, the blue precipitate is formed likewise; but, instead of constituting a hidrat, it passes to green, without the contact of air being capable of obscuring its tint, which it retains after it is dried.

Changed to a grey by boiling.

3. If this green precipitate, when fresh made, be boiled in water alkalized with potash, it becomes of a reddish grey, and changes no further.

Action of acids on the precipitates.

Very weak acids, as vinegar for instance, totally dissolve the first precipitate. Applied to the other two, they separate from it black oxide. Lastly, the blue oxide yields no gas with muriatic acid, but the green does.

Green oxide a compound of the blue and black.

Hence we must conclude, that the blue oxide oxigenizes itself at the expence of the air contained in cold liquors, and that the green oxide is a mixture of blue oxide and black oxide. Mr. Proust however thinks, that something more than simple mixture takes place; for blue and black would not produce that grass green colour, which distinguishes it from every other oxide. A true combination alone could form a colour different from that of the mixture of its component parts, and prevent the action of the air from raising to a maximum the portion of blue oxide, which makes a part of the green precipitate. To oxide this precipitate completely, it must be dried with the assistance of heat, as Thenard shewed.

The reddish grey precipitate of the third experiment is a mixture of hidrat and black oxide.

Maximum oxide only soluble.

The oxide at a minimum only is capable of combining with acids. The green oxide is never obtained from any solution, and cannot become the base of any saline combination.

Ammonia and Oxide of Cobalt.

Dissolves with difficulty in ammonia,

If the grey oxide be enclosed with ammonia in a well stopped phial, it imparts to it a slight rose colour, which does not become deeper, however long it may be kept. This oxide is consequently very difficultly soluble in ammonia.—

readily in its carbonate.

But if the phial be left open, the ammonia becomes coloured very quickly, because it attracts carbonic acid from the air. This solution may be effected in a very little time, by placing the phial in a large jar containing a carbonate.

If

If the ammonia be merely saturated with carbonic acid, the liquid will be a solution of oxide of cobalt in carbonat of ammonia: but if we continue to pass corbonic acid into this solution, we obtain a solution of carbonat of cobalt in carbonat of ammonia. This solution, kept in a full bottle corked, deposits crystals of metallic carbonate; it likewise lets fall a part on the addition of water; but an excess of ammonia redissolves this precipitate. This a solution of the oxide, or of carbonat.

This solution may be made very quickly, by throwing carbonat of cobalt into carbonat of ammonia. Made directly.

If pure ammonia be poured on carbonat of cobalt with excess of acid, what occurs is very different. The carbonat of cobalt separates into two parts; one gives out its acid to the ammonia, and becomes a hidrat, which falls to the bottom of the vessel; while the portion not decomposed dissolves in the carbonat of ammonia. Ammonia with carbonat of cobalt,

Thus we have two kinds of ammoniacal solutions of cobalt; and there is a third, which Taffaret observed, but which has been hitherto little noticed. This is obtained by putting well washed hidrat, or blue oxide, into a phial full of ammonia, and closely stopped. A solution will take place in the course of four and twenty hours. This is red, like the preceding; but differs from them in this, that, if it be poured in a very slender stream into boiling water, blue oxide will immediately be precipitated; if into cold water, green oxide will be obtained. If ammonia dissolve hidrat of cobalt, or blue oxide fresh made, more readily than the gray oxide, it is because they are in a state of extreme division. and with hidrat, or blue oxide.

Distillation of ammoniacal solutions.

When carbonated solution of cobalt are distilled, carbonate of ammonia passes over, and at length the liquor lets fall an oxide, which is at first of a dirty green, but which afterwards becomes black. This is a mixture of the gray and black oxides. The carbonated solutions distilled leave the cobalt more oxided.

How is this superoxidation effected? The author reports the facts, but does not endeavour to explain them, when data are wanting.

Hidrat of Cobalt.

Hidrat of cobalt,

Crystals of sulphat or nitrat of cobalt, thrown into a bottle filled with a solution of potash, and immediately corked up, are decomposed. A blue precipitate is formed, which changes to a violet, and afterwards to a rose colour, becoming a hidrat.

decomposed by potash,

If the hidrat of cobalt be boiled with potash, this dissolves some oxide, and acquires a fine blue colour. This solution is decomposed by the addition of water. By exposure to the air the oxide becomes black, and falls down.

and dissolved in its carbonat.

Hidrat fresh made dissolves without heat in carbonat of potash, and tinges it red. The oxide does not dissolve in it.

Characters of the hidrat.

The hidrat of cobalt is of a rosy feuillemorte colour. Acids dissolve it with heat, and without effervescence.

The hidrat is not decomposed by boiling either in pure or in alkalized water. Heat expels from it 20 or 21 of water, and reduces it to very pure gray oxide.

It does not keep well under water; when it is exposed to the contact of air, it grows black. Dry hidrat keeps better, but it attacts carbonic acid.

Dissolves in ammonia as it forms from the sulphat.

When crystals of sulphat of cobalt are thrown into a phial full of ammonia, which is immediately closed, they yield a blue precipitate, which does not become rose coloured, as in potash. Mr. Proust affirms, that the hidrat is formed, but that as fast as it is produced it dissolves in the ammonia; so that it is the hidrat that colours the solution, and not the simple oxide.

Estimation of the quantity of oxigen in the oxide at a minimum.

Gray oxide contains 16·5 per cent. of oxigen.

A hundred parts of gray oxide, reduced with the requisite precautions in a closed crucible, afforded 83·5 of metallic grains. One hundred parts of the metal therefore appear to absorb nineteen of oxigen, to become oxide at a minimum.

Oxide at a maximum.

Black oxide 20 per cent.

If a nitric solution of cobalt be distilled, black incrustations will

will be deposited on the sides of the retort, nitrous gas will be evolved, and the residuum obtained will be black oxide. The quantity will be in the proportion of 125 or 126 parts to 100 of the metal in the solution. Hence we may infer, that the maximum of the oxidation of cobalt is between 25 or 26 of oxigen to a hundred parts of the metal.

This oxide is not soluble either in the nitric or sulphuric acid, without losing that portion of oxigen, which converts it to the state of a maximum. Insoluble in acids without losing oxigen,

With muriatic acid it gives out oxigen gas.

It is insoluble both in ammonia and in potash. and in alkalis.

The black oxide, heated for half an hour at the bottom of a retort, returns to the state of gray oxide by parting with oxigen; and it is then capable of giving a blue tint to vitrifiable substances. Heat converts it to gray oxide.

Messrs. Proust and Thalaker have met with the black oxide at Pavias, a day's journey from Valentia. It is found likewise in those ores of cobalt which are termed irtreats, or black ores. Native.

The corbonat and hidrat of cobalt are changed into black oxide by oxigenized muriatic acid. From carbonat and hidrat.

The nitrous and sulphurous acids dissolve the black oxide, forming with it nitrat and sulphat at a minimum. Soluble in nitrous and sulphurous acids.

Muriat of Cobalt.

The gray oxide disolves with heat in muriatic acid of 15°. The solution, whether hot or cold, is of a deep blue: it crystallizes easily, and the crystals are blue: this is the anhidrous muriat. As soon as it has absorbed moisture, it becomes red. Blue, or anhidrous muriat, and red.

Muriatic acid of 15° yields much gas with the black oxide. This solution is green as long as it retains any gas; but as soon as it has lost it, it becomes blue. The blue traces of muriat of cobalt dried on paper are anhidrous muriatic. When they are green, it is because the salt still contains muriat of nickel, which gives a yellow tinge, and thus forms green with the blue. Green muriat contaminated with nickel.

Its distillation.

Urged to redness in a luted retort, it is decomposed only in the Heated, what touches the glass is decom-

posed, the rest sublimes.

the parts that touch the glass. The products then are muriatic acid gas mingled with oxigenized acid. The glass becomes tinged with blue. The muriat that is not decomposed sublimes, after having melted, in flowers of a gridelin colour. These flowers have acquired a kind of condensation, which renders them insoluble in water for twelve hours at least: but at length they afford a solution of ordinary muriat.

Arsenit and Arseniat.

Arsenit, how prepared.

The Arsenit of cobalt is prepared by pouring a very dilute solution of cobalt into a solution of arsenit of potash. A rosy precipitate is formed, which retains this colour after desiccation.

Character of the Arsenit.

Its characters.

1. Heated in a tube closed at one end it is decomposed; the oxide of arsenic sublimes; and the glass is tinged blue.

2. The nitric acid dissolves it, and nitrous gas is evoled.

3. Its solution in muriatic acid is decomposed by sulphuretted hidrogen, which precipitates orpiment.

4. Caustic potash, assisted by heat, separates the blue oxide.

Arseniat.

Arseniat.

This is obtained by employing arseniat of potash, instead of arsenit. The precipitate is rose-coloured like the arsenit.

Its Characters.

Its characters.

1. Heated in a tube it yields no sublimate, and becomes violet-coloured, without tinging the glass.

2. Nitric acid dissolves it without giving out nitrous gas.

3. Its muriatic solution does not become turbid by the addition of sulphuretted hidrogen in less than two hours after they are mixed.

4. Caustic potash separates blue oxide and combines with the acid.

Native.

The rosy efflorescences found or minerals containing cobalt consist

consist of arseniat. Mr. Proust found arseniat only in the interior parts of some fragments.

Hidrosulphuretted oxide. Sulphuret of Cobalt.

The gray oxide, the hidrat, and the carbonat, take sulphuretted hidrogen from water, and become hidrosulphuretted oxide. This is not soluble in ammonia. By distillation it gives out water and sulphurous acid, and the residuum is a sulphuret. Hidrosulphurets, and sulphuret.

The oxides heated with sulphur are converted into sulphurets.

One hundred parts of cobalt absorb forty of sulphur; though Mr. Proust has some doubts respecting this proportion.

Facts toward a history of Nickel.

Nitrat of Nickel.

A hundred parts of the metal dissolved in nitric acid, and distilled till they are completely decomposed, leave 125 or 126 of greenish gray oxide at a minimum. Nitric acid cannot convert this oxide to the maximum state. Nitrat of nickel.

To ascertain the purity of the oxide of nickel, it must be dissolved in muriatic acid, and exposed to the action of heat. If it contain a little oxide of cobalt, oxigenized muriatic acid gas will be evolved; if it be pure, more will be given out. Test of its purity.

The gray oxide dissolves in all the acids, and affords the same solutions as the metal itself.

Nitrat at a Minimum.

If nitrat of nickel be distilled with the same precautions as nitrat of copper, we obtain, as with the latter, a nitrat with excess of base, which is insoluble in water. Of this nitrat 142 parts are afforded by 100 of nickel; so that, if we substract the 25 parts of oxigen, which the metal has absorbed, we have 17 parts of acid combined with the oxide. Minimum nitrat, nickel 70·4, oxigen 17·6, acid 12.

Nitrate

ish gray oxide at a minimum. When heated in contact with the air, the oxide is black.

The minimum oxide is converted into carbonat by exposure to the air.

Hidrat of Nickel.

Hidrat. All the salts of nickel, thrown into a boiling solution of potash, are converted into a green hidrat. Boiling does not alter its colour. Potash does not dissolve either the hidrat or oxide of nickel.

The hidrat is reduced to gray oxide by heating.

In this state in all its salts. In the saline combinations the oxide is in the state of hidrat. Alkalis precipitate it in this state.

Maximum oxide of Nickel.

Oxide at a maximum. The carbonat and hidrat are both oxided to a maximum by the action of oxigenized muriatic acid. The gray oxide is more difficult to oxide.

Black. The dry oxide of nickel at a maximum is black. When solid its fracture is vitreous.

Oxigen expelled by ammonia. This oxide kept in ammonia gives out bubbles, returns to the state of gray oxide, and dissolves in the alkali.

With muriatic acid at 15° it yields a considerable quantity of oxigenized acid. The solution is greenish yellow, and crystals form in it by cooling.

Oxides reduced. The oxides of nickel are reduced like that of cobalt. The metal is obtained pretty easily in a button, in which it differs from cobalt, this affording only large grains.

Sulphuret. This metal appears to have taken up a surcharge of sulphur of 46 per cent.; but Mr. Proust entertains some doubt of the accuracy of this proportion.

Arsenit and Arseniat.

Arsenit. These are made like those of cobalt, and are of a fine apple-green colour.

The arsenit, heated in a tube, loses its colour with its water, gives out white oxide, and changes to an olive green. To abstract all the arsenic the contact of charcoal is requisite.

Heated

Heated under charcoal in a spoon of platina, the arsenic is quickly dissipated, and oxide at a minimum remains.

Arseniat. The arseniat, heated in a tube, loses its colour with its water; becomes transparent and of a hyacinth red; but if the heat be carried to redness it turns of a pale yellow, and remains unalterable.

In the spoon the arseniat turns white, and grows red hot without melting, or emitting the least arsenical fume. To decompose it an obscure flame is required.

Recapitulation.

Most metals have but two oxides, From the preceding facts, and others which he has given in different memoirs, Mr. Proust concludes, that cobalt, nickel, and most of the well known metals, have but two very decided degrees of oxidation. He does not mean to say, that a metal can absorb oxigen in too proportions only: he only asserts, that it is too soon to admit all the oxides which have been mentioned by chemists, and of which neither the quantity of oxigen, nor the combinations they are capable of forming with acids, can be considered as determined; and that colour is not a character sufficient to constitute a distinction.

at least that are yet known,

The only excepting tin and lead. There are but too metals, that have hitherto afforded him more than two oxides. These are tin and lead. However, the quantity of oxigen in that oxide of tin, which constitutes the base of mosaic gold, is not yet known; or that of the oxide of nitrat of lead made by boiling with plates of this metal.

Oxides of the same metal continue chemically. It seems, that the different oxides of the same metal can mutually dissolve each other, and form true combinations. Thus the green oxide of cobalt is a combination of the blue and black oxides,

Minimum perhaps a compound. Is not minimum a combination of the brown oxide of lead and oxide at 9 per cent. in a similar manner?

Magnetic iron ores such compounds. Finally, all the magnetic ores of iron, and magnetic sands are mixtures or combinations of this order. If this were not the case what would prevent the minimum oxide from being raised to a maximum of oxidation? The oxide of a gun-barrel that has been used for decomposing water is likewise in a similar state, it is composed of two oxides.

XI.

Facts toward a History of the Gallic Acid. By BOUILLON LAGRANGE.*

I HAVE had the honour of submitting to the class the result of my experiments on tannin; I now lay before it some facts respecting the gallic acid, which I had announced as forming the second part of my memoir.

History of the gallic acid.

Of all the vegetable acids, the gallic may be considered as most interesting, and accordingly it has been a subject of inquiry to many chemists. Macquer, Monnet, Lewis, Cartheuser, and Gioanetti, pointed out the manner, in which solutions of iron are acted upon by substances called astringent. The academicians of Dijon were the first, who observed the presence of an acid in those subtances; and in 1772 they shewed, that the distilled products of nutgalls blackened the solution of sulphat of iron, and that an infusion of them reddened the tincture of bitmus. These particulars afforded only a general proof of the acid nature of the principle contained in galls; offering no means of extracting this acid, and obtaining it separate, for which we are indebted to Scheele. His process was published in 1780. A few years after, in 1793, Mr. Deyeux discovered that this acid might be obtained by sublimation. Messrs. Berthollet and Proust afterward added much by their researches to our knowledge of the properties of this acid; so that it might be considered among the best known of all that the vegetable kingdom produces.

Scheele first extracted it. Deyeux sublimed it.

Several foreign chemists too, within these few years, have given processes for extracting and purifying this acid: but none of them, except Richter's, can come in competition with Scheele's. Among the many experiments, that have been made on this subject, there is one, which I have neither seen refuted nor quoted in the papers published on the gallic acid.

Some facts by Bartholdi unnoticed.

In a letter from Mr. G. C. Bartholdi to Mr. Berthollet, dated 1792, there are some facts, that might have claimed the attention of chemists.

* Annales de Chimie, Vol. LX. p. 156, Nov. 1806.

Mr. Bartholdi first points out a process for obtaining pure gallic acid: he afterward treats this acid with metallic oxides; and he says he has demonstrated, that all substances, which yield oxigen to the gallic acid, give it a brown colour; and that, in this process, it is the acid itself, which, being charred, forms by a slight combustion the colouring matter.

Oxiding substances brown the gallic acid by slightly charring it.

To show this, he boiled red oxide of mercury for half an hour in a solution of gallic acid, which assumed a blackish hue. In the residuum he found fluid mercury, mixed with a coally powder: he afterward saturated the liqour with carbonate of potash and soda, and the salts thus produced afforded no blue precipitate with sulphat of iron.

Boiled with oxide of mercury carbon was produced, and it would no longer colour iron.

He obtained the same result with oxide of manganese.

Oxide of manganese the same.

Other experiments convinced Mr. Bartholdi, that substances, by which oxigen is abstracted from gallic acid, renders its colour lighter. I rendered, he says, a solution of gallic acid as limpid as distilled water, by boiling it for some time with very pure and well powdered charcoal, of which I took double the weight of the acid: it retained its limpidity as long as I excluded the influence of the atmospheric air from it, and it precipitated iron black.

Disoxigenizing substances whiten gallic acid.

Mr. Bartholdi presumes, that we may thus effect the destruction of its astringent property.

Its astringency thus destructible.

On this I shall not for the present make any observations; as it is necessary to be acquainted with the following experiments, to judge them explicitly.

Extraction of the Gallic Acid.

There are several processes for extracting the acid from galls.

Modes of extracting the gallic acid.

Scheele's process.

Scheele's.

On one part of gall-nuts bruised and passed through a coarse sieve, pour six parts of cold water. Let them macerate in a glass jar four days, shaking them frequently: then filter, and expose the liquor to the open air in the same jar, covered only with blotting-paper. In a month's time the liquor will be covered with a thick pellicle of mould, without any precipitate being

being formed; and it will have lost its astringent taste, but be acid. On leaving the liquor at rest five weeks longer, a precipitate will be formed two fingers thick, and a mucous pellicle above it. The liquor is now to be filtered again, and left anew exposed to the air. At the expiration of some months, the greater part of the liquor will be evaporated: all the precipitates are to be added together, and cold water is to be poured on them; when the liquor has stood to settle, what is clear is to be decanted off: as much hot water as is necessary for the solution is poured on; and by evaporating with a gentle heat yellow crystals will be obtained.

Mr. Bartholdi's process.

Bartholdi's. A tincture of galls in alcohol is to be evaporated; the residuum is to be dissolved in distilled water; and sulphuric acid is to be added to the solution, till the mixture is decidely acid to the taste. In the course of a few hours the extractive matter will fall down, and the supernatant fluid, freed from sulphuric acid by barytes, will yield, according to the author, pure gallic acid.

This process by no means gives this result. It is in general very difficult to seize the moment when all the sulphuric acid is removed by the barytes, since it combines with the gallic acid likewise: and after the liquor is evaporated nothing remains but an acerb matter, containing a great deal of tannin, and insusceptible of crystallization.

Process of Mr. Deyeux.

Deyeux. This chemist discovered, that, by heating bruised nutgalls slowly, and cautiously, in a glass retort, a pretty considerable quantity of lamellated, shining, and silvery crystals was sublimed.

Mr. Richter's process.

Richer's. Nutgalls reduced to a fine powder are to be macerated in cold water, shaking the mixture frequently. After some time the liquor is to be strained off through a cloth: the residuum is to be macerated in a fresh portion of water, and after it has been

been subjected to the press, the two liquors are to be mixed, and evaporated by a very gentle heat. Thus we obtain a dark brown substance, very brittle, which being reduced to a fine powder, and digested in very pure alcohol, tinges it of a very faint straw colour. A second infusion extracts scarcely any colour, and leaves a brown residuum, which is tannin nearly pure. The two alcoholic tinctures are then to be mixed, and distilled in a small retort, till seven-eighths have passed over. The remaining liquor on cooling becomes nearly solid: water is poured on this, and by gently heating a limpid solution is obtained with very little colour.

If this solution be evaporated, very small and very white prismatic crystals are obtained. The mother-water affords yet more, but they are commonly a little coloured: these however, may be rendered very white by washing them with water. By this process a pound of galls affords half an ounce of crystals. They are extremely light.

Gallic acid prepared in the humid way contains tannin.

The processes of Scheele, Deyeux, and Richter, have afforded advantageous results; but they differ with respect to the purity of the acid. The acid produced by the first, as Berthollet observes, retains a great deal of tannin; that by the second is perfectly white; that by the third likewise contains tannin.

In Richter's process too frequent solutions in alcohol decompose the acid.

By Richter's process the acid, after being purified, is of a pale straw colour. I attempted in vain to bring it to the state of purity mentioned by the author. I found, that if the evaporation, desication, and subsequent solution in alcohol were continued, a certain quantity of acid was decomposed every time; so that the alcoholic tincture, instead of being more transparent, became brown. There is a certain point therefore, where we must stop, if we would preserve the whole of the acid and its properties.

Berthollet purified it by oxide of tin.

Mr. Berthollet tried different modes of purifying Scheele's acid. That which succeeded best with him was treating the acid with oxide of tin recently precipitated from its solution in an acid.

This repeated by the author.

This experiment I repeated. The following is the method I pursued, and the phenomena I observed.

After having separated the oxide of muriat of tin by an alkaline

alkaline base, I washed it well with boiling water, and then boiled it for some time in a fresh quantity of water. I then treated it with gallic acid, and evaporated to the consistence of thick honey. I then added distilled water: and the liquor, after being filtered, was colourless, limpid, without taste, and without smell. On evaporating to dryness nothing remained.

without process.

This difference from the result obtained by Mr. Berthollet led me to suspect, that I had fallen into some error. I therefore repeated the experiment with all the attention possible.

Further trial.

I dissolved 61 grammes of gallic acid, confusedly crystallized and very brown in 500 grammes of boiling water. Part of this solution I set by as a standard of comparison: the rest I boiled with 61 grammes of oxide of tin well washed, and still wet. When about half the liquor was wasted, I made it up to its original weight with fresh water, compared it with the standard, and found it had lost a great deal of its colour. The difference of acidity was scarcely perceptible. It still precipitated glue: but the precipitate was yellow and floculent, while that of the solution not purified was brown, heavy, more copious, and even united in a mass. It appears, that the acid was not yet decomposed: but I could not obtain crystals equally white and pure with those afforded by sublimation, as Mr. Berthollet did.

Did not succeed.

More oxide decomposed the acid.

Desirous of knowing whether a fresh quantity of oxide of tin would deprive the acid of tannin entirely, I added to the liquor 30 grammes of oxide of tin, and evaporated till about 100 grammes of liquor only remained. It passed through the filter clear and colourless, and precipitated neither sulphat of iron nor glue. I could not obtain any gallic acid by evaporation.

This experiment proves, that it is very difficult to free gallic acid completely from tannin; and that by repeating the action of oxide of tin the acid is decomposed. Thus no doubt Mr. Proust proceeded; for this chemist observed, in his memoir printed in the *Annales de Chimie*, vol. 42., that the oxide of tin he employed to purify the gallic acid afforded him as a product only a colourless insipid liquor, without taste, and not having the slightest effect on solutions of iron or tincture of litmus.

Proust found the same,

Bartholdi's experiments repeated.

As to the means proposed by Mr. Bartholdi, I do not imagine they can be employed. Yet, as the author neglected to examine the products of his operations, I thought it necessary

to

to repeat his experiments, and determine the nature of the results that might arise from them. With this view I poured a solution of gallic acid or red oxide of mercury; which immediately became brown, and gradually changed to black. The solution too acquired a deep brown tint. In this state it was still acid, gave a blue colour to a solution of sulphat of iron, and precipitated glue; but it contained no mercury. With red oxide of mercury.

I boiled this liquor on a fresh quantity of oxide; when it became clear, colourless, and no longer contained either tannin or gallic acid.

Part of the oxide of mercury was reduced: the rest was mixed with concrete phosphoric acid [so the original], but nothing was sublimed from it by the action of lead.

If charcoal previously purified be employed instead of red oxide of mercury, the solution of gallic acid loses almost entirely its taste and colour; the liquor becomes green, and no longer precipitates glue; but it still gives a violet blue tint to solution of sulphat of iron. Boiled with a fresh quantity of charcoal, the liquid becomes colourless, and no longer produces, any change in the solution of glue or of sulphat of iron. After it is evaporated to dryness, a brown matter remains in the capsule which precipitates acetat of lead of a dirty gray, and nitrat of mercury and muriat of tin yellow; so that we may consider it as extractive matter. With charcoal.

These experiments prove, that there exists no process for purifying Scheele's gallic acid but sublimation; unless the proportion of oxide of tin employed by Mr. Berthollet, which he does not mention, has a great influence on the result. Yet the mode of purifying the gallic acid by sublimation cannot be adopted, if we wish it to retain all its properties. The different characters exhibited by the two acids will afford proofs of this assertion. Only to be purified by sublimation, but this alters its properties.

Comparison of the crystallized and sublimed Gallic Acids.

Scheele's crystallized acid imparts to water a slight lemon colour: this solution grows deeper coloured by the action of the air: it reddens tincture of litmus: limewater produces in it a blue colour, which changes to that of peach blossoms if the limewater be in excess, and on adding a few drops of nitric acid to a rose colour. The same phenomena take place with water of barytes. Solution of the crystallized acid, with lime water, barytes,

This

alkalies, This solution takes a colour more or less green with carbonat of soda, but is not changed by carbonat of ammonia. Caustic potash changes it to a deep brown; and ammonia to a reddish brown.

sulphat of iron, nitrat of mercury. acetat of lead, muriat of tin, With green sulphat of iron it is a violet blue, which is constant, as an excess does not alter it. With nitrat of mercury it gives a yellow precipitate; with acetat of lead, and muriat of tin, a white.

oxigenized muriatic acid, The solution of this acid is not altered in appearance by oxigenized muriatic acid.

and glue. With glue it gives a copious precipitate.

Richter's acid. The same experiments were made with the acid obtained by Richter's process, and the results were similar, except that the precipitate thrown down by glue was very abundant.

Solution of sublimed acid, *Sublimed acid of Deyeux.* The solution of this acid by hot water emits an aromatic odour, and a slight oily pellicle is perceptible on its surface.

This solution becomes brown by exposure to the air. It

with lime-water, barytes, faintly reddens tincture of litmus: limewater gives it a colour of wine lees, which an excess of it converts to a fawn colour. With barytes we obtain the latter tint, and the liquor is immediately covered with an oily pellicle.

carbonated and Carbonat of ammonia produces no change in the acid liquor: that of soda gives it a fawn colour.

pure alkalies, Caustic potash browns it considerably: with ammonia the colour is lighter.

sulphat of iron, If a few drops of a solution of sulphat of iron be dropped into this acid liquor, a blue colour is produced, which soon changes to a violet blue. Frequently however, instead of a blue colour, we have a deep green. This no doubt depends on some peculiar circumstances: and I conceive it may be attributed to the degree of oxidation of the iron; for with muriat of iron at a maximum we have constantly a green colour. This effect is less striking with other acids: the infusion of galls, made without lead, always retains its pure blue colour.

With nitrat of mercury the precipitate is blackish: that with

nitrat of mercury, acetat of lead, acetat of lead is fawn coloured, and very light.

other metallic salts, The sulphats of zinc and copper, and muriat of tin, produce no change.

Oxigen-

Oxigenized muriatic acid browns the solution of gallic acid, and an excess deprives it of colour. and oxigenized muriatic acid.

On comparing the difference of the effects of these acids, it will be easy to appreciate them.

The sublimed acid has less acidity: it is decomposed by the air: it has no action on barytes, carbonat of ammonia, or muriat of tin. The precipitate obtained with nitrat of murcury is blackish, instead of yellow: that with acetat of lead is slight and fawn coloured, instead of copious and white. The sublimed acid compared with the crystallized.

Oxigenized muriatic acid browns a transparent and colourless solution of the sublimed acid, while it does not alter the colour of a solution of the crystallized.

Lastly the sublimed acid does not constantly produce the same colour with sulphat of iron, and does not precipitate glue.

If it be easy to point out the characters that distinguish these two acids, it is difficult to explain whence their difference arises. Mr. Berthollet has justly observed, that Scheele's acid, when not purified, contains a great deal of tannin; and that, when purified by oxide of tin, it does not percipitate glue. Not easy to account for the differences.

As to that of Mr. Richter, I have already pointed out its analogy to Scheelés: yet both these acids appear to me to differ from that obtained by sublimation. The latter contains a small quantity of volatile oil, which is combined with it; and which by the action of caloric assumes a character approaching to that of oils rendered resinous. This property may be ascertained by dissolving the sublimed acid either in alcohol, or in ether; for if the liquid be evaporated by rubbing it on the skin, we shall experience an effect similar to that produced by a resin dissolved in alcohol. Sublimed acid contains a volatile oil.

It is not without difficulty, as may be supposed, that we can attain a complete knowledge of the nature of the gallic acid. Does this acid exist in galls already formed? May we consider it as a peculiar acid; or rather is it merely the result of the combination of a vegetable acid with tannin, extractive matter, and other substances existing in galls? These are questions, that yet remains to be solved. I have attempted by a series of experiments to add some facts to those that are known; and if they do not yet lead to a complete solution I conceive some new results will be found in them, which serve to explain the nature and properties of the gallic acid. What is the gallic acid?

Examination of the Action of Caloric and of Water on Nutgalls.

Action of Caloric.

Action of caloric.

Mr. Deyeux having examined in a particular manner all the products of the distillation of galls on a naked fire, I shall consider only the acid liquor obtained from them.

Properties of the distilled fluid.

The process was conducted in the manner indicated by that chemist. The fluid in the receiver was aromatic, a little milky, very acid, did not precipitate glue, and gave a violet blue with sulphat of iron, which changed to a dirty green. Lime and barytes produced a peach-blossom colour. Nitrat of mercury threw down a blackish precipitate; acetat of lead, and muriat of tin, a white.

Saturated with potash, gave signs of acetic acid.

Having saturated the acid liquor with potash, I obtained by evaporation a brown empyreumatic matter, which on the addition of sulphuric acid emitted a pungent smell resembling that of acetic acid.

Action of Water on Nutgalls.

Gall macerated in water.

Galls finely powdered being shaken in cold water for four minutes, the liquor, when filtered, was of a golden yellow colour. One part was distilled in a retort on a sand heat: the other was saturated with carbonat of soda.

Distilled gave an acidulous fluid.

The produce of the distillation was a clear, colourless, and slightly acid liquor, that precipitated neither glue nor sulphat of iron.

Saturated with an alkali, sulphuric acid added and distilled.

The liquor saturated with the alkali was evaporated to dryness; and the residuum being dissolved in distilled water, sulphuric acid was added till it was a little in excess, when the mixture was distilled in a retort. The products were examined in succession. First a fluid came over without taste or smell: soon after the liquor was acid, but contained neither sulphuric nor gallic acid.

With boiling water.

I made a similar experiment with boiling instead of cold water. The liquor remained turbid, though filtered. Being subjected to distillation, and combined with soda, in the same manner as the preceding, I obtained the same results.

These

These experiments suggested to me the existence of a acid ready formed in galls, and the possibility of obtaining it by distillation. A free acid in galls.

Accordingly I heated to ebullition in a common alembic a kilagramme [2lb. 3oz. 6dr. avoird.] of galls coarsely powdered, with double the weight of water. The distilled liquor, as Mr. Deyeux observed, was a little milky, aromatic, and on standing deposited a little floculent sediment. I changed the receiver, when about two thirds of the liquor had come over, and I continued the distillation till it became coloured. Obtained by distilling with water.

The first product was acid; reddened tincture of litmus; and had no action on lime or barytes water, nitrat of mercury, acetat of lead, sulphat of iron, or glue. 1st. product.

The second product was turbid, coloured, a little empyreumatic; its acidity was more marked; and it precipitated the metallic solutions above mentioned, but did not act on glue. 2nd. product.

Each of these acid liquors was saturated with potash. The first yielded a foliated salt, which, on the addition of sulphuric acid, gave out a smell of asetic acid. Part of this salt was dissolved in distilled water: the excess of its base was accurately saturated by nitric acid, and nitrat of mercury at a minimum was added to the solution; when a precipitate was formed, which had all the characters of acetat of mercury. To convince myself still farther of the presence of acetic acid, I treated the neutral acetat of potash in the same manner, and it afforded me the same results. Saturated with potash. Proofs of the acid being the acetic.

The second product was saturated with potash in the same manner. The liquor became very brown: a slight pellicle formed on the surface, which increased during the evaporation: the saline matter was highly coloured and empyreumatic. Being subjected to the same trials as the preceding, similar appearances were observed. 2nd. product afforded similar proofs.

These experiments leave no doubt of the presence of acetic acid in galls: they prove, that it may be obtained by distillation with water; and that caloric, when it acts more directly on this acid, facilitates its combination with a small quantity of empyreumatic oil, and perhaps with a little tannin, the presence of which is not demonstrable by glue: but as this liquor acts on sulphat of iron in the same manner as the sub- The acetic acid. by means of caloric is combined with an empyreumatic oil, and perhaps tannin,

and with aromatic oil, instead of empyreumatic, when sublimed.

limed acid, we must presume, that there is a kind of analogy in their composition; admitting however this difference, that the sublimed acid contains no empyreumatic oil, but a particular aromatic volatile oil.

The oil shown.

This oil may be detected by dissolving the acid in very pure sulphuric ether, and adding a little water, when a few drops of oil will be seen floating on the surface, which disappear on shaking the mixture.

Caustic potash.

If a concentrated solution of caustic potash be employed instead of water, a white, milky substance is separated, which requires a large proportion of water to dissolve it, but the liquor still remains turbid.

The ethereal tincture,

This ethereal tincture yields a fine blue colour with sulphat of iron.

and its residuum.

Evaporated in the open air it leaves a shining substance, very acid, separating in scales, and having the appearance of a varnish.

The same phenomena take place, if galls be digested in ether; but the substance contains tannin in addition.

Examination of some earthy and alkaline gallats.

Farther proofs of acetic acid

Though it appears to be demonstrated, that acetic acid exists ready formed in galls we cannot too much multiply proofs in confirmation of its presence; and to demonstrate, that this acid, combined with other substances, constitutes the gallic acid.

by forming different gallats.

With this view I formed gallats of lime, barytes, potash, and soda. These neutral combinations afforded a violet red colour with a solution of sulphat of iron, and scarcely precipitated glue while the acid employed had the property of forming with it a copious precipitate. On these salts dried I poured some very weak sulphuric acid; I distilled them with a gentle heat, and I always obtained acetic acid.

The residuum afford no proof of a gallic acid.

The retorts contained a very deep brown matter. I crystallized the salts that were preceptible of it, and obtained sulphats.

The supernatant mother-water had the property of slightly browning the solution of sulphat of iron; but this appearance does not prove the presence of gallic acid, for the black colour of the mother-water was sufficient to give it this hue.

If one of the gallats, that of soda for instance, be treated with charcoal, the tannin will be entirely destroyed, so that the solution will no longer precipitate glue; and after repeated boiling with fresh portions of charcoal, it will no longer act on sulphat of iron. Gallats treated with charcoal,

The liquor being afterward evaporated to dryness, and distilled with very weak sulphuric acid, we still obtain acetic acid. still give acetic acid.

I shall not insist any farther on the possibility of obtaining acetic acid by decomposing gallic acid. I might mention the experiments, which would tend to support the preceding, but entering too minutely into these particulars would add nothing to the facts I have already adduced.

I shall conclude with an experiment, which appears to me important. The object was, to establish the nature of the elastic fluids resulting from the complete decomposition of the gallic acid by heat. Mr. Deyeux has announced, that he obtained only oxigen gas, and carbon. Mr. Berthollet, who repeated the experiment, says, that he had no oxigen gas, but constantly carbonic acid. Examination of the gasses produced by decomposing gallic acid.

These results, of which no other vegetable acid furnishes an example, could not avoid exciting the attention of chemists. In fact it is difficult not to admit hidrogen in the composition of gallic acid; and Mr. Fourcroy has expressed his doubts on this subject in his System of Chemical Knowledge, but the question is not yet decided by experiment.

In consequence I heated gallic acid in a retort. The fire was gradually raised till the retort was red hot. During this action of caloric I obtained several jars full of elastic fluid. The first contained only atmospheric air; the others carbonic acid gas: at least the gas had all the characters of thin acid; but the phenomena that occurred during the decomposition of the gallic acid led me to suspect, that, if any hidrogen gas had been evolved, it could exist only in a very small quantity. I did not satisfy myself therefore with the trial by limewater, and the extinction of a taper in the gas. Having perceived, that hidrogen gas mixed with a great deal of carbonic acid gas cannot be fried, because this acid acts too promptly on the flame of the taper, I passed a little caustic potash into the last jar of gas; agitated it, absorb the carbonic acid; and then immersed a taper in the residual gas, which burnt with flame, and thus afforded me a proof of the presence of carburetted hidrogen. The first portion the air in the retort, afterward carbonic acid gas, mixed with carburetted hidrogen.

The

Base of gallic acid, hidrogen and carbon.

The gallic then, like other vegetable acids, is composed of oxigen, hidrogen, and carbon. If but a small quantity of hidrogen, can be obtained, it is because water is formed during the decomposition of the acid, so that the hidrogen passes over only when very little oxigen remains to act on the carbon.

I have attempted to shew, that the gallic acid is a compound. Its formation by Scheele's process appears to me to favour this opinion. In fact, if the quantity of acid extracted from the aqulous infusion exposed to the air be compared with that afforded by sublimation, I conceive it is not difficult to account for the increase. There can be no doubt, that acetic acid formed in the liquor, which, acting on a portion of tannin and extractive matter, constitute the gallic acid of Scheele: but this combination is rendered more intimate, and somewhat different, by the action of caloric; of which we have a proof when the acid is obtained by sublimation, for not only is the tannin decomposed, but the acid remains combined with a volatile oil which is formed. Perhaps this acid contains a small portion of tannin in very intimate combination, whence no doubt arises its property of giving a momentary blue with sulphat of iron, though its presence cannot be demonstrated. This acid then must have different properties from that of Scheele: and if it were possible to assimilate it to other vegetable acids, the benzoric would it be that, which it would have the greatest analogy. May it be considered however as a modification of the gallic acid? I think not. It is the same with other vegetable acids: and it is probable, that there exists no modification of them. The acetous appears to be the sole vegetable acid: it desolves and retains in various proportions a number of the immediate products of vegetables, and in the processes to which we subject vegetable substances, we facilitate its combination in a more intimate manner; and frequently even augment the quantity of this acid. Already several chemists have admitted the possibility of the acetic acid's dissolving and remaining combined with fixed and empyreumatic oils, and animal matters; they have even gone so far as to imitate acids of this sort. The formic, pyrolipic, pyrotartarous, and pyromucous, have been classed by Messrs. Fourcroy and Vauguelin among the compound acids: it is the same with the lactic,

Acid of Scheele.

Sublimed acid,

not a modification of the gallic.

Acetous the only vegetable acid, forming others by various additions intimately combined with it.

Instances.

lactic, the composition of which was pointed out at the same time by those chemists, Mr. Thenard, and myself: lastly we have proof too, according to Mr. Thenard, of the existance of this acid in the urine and sweat, as well as in the sebacic and ozonic acids. I might farther add to these observations (if we were not persuaded, that the acetic acid is found every where) that it exists in the vegetable as in animal matters, where it is almost always in a state of combination; and that, an equilibrium in the proportions being once established, it gives rise to compounds hitherto unalterable, and the affinity of which cannot be destroyed, but by reducing them to their primary elements, oxigen, hidrogen, carbon, and nitrogen.

From the facts announced in this memoir it follows:

Recapitulation.

1st, That the gallic acids of Scheele and of Richter differ essentially from that obtained by sublimation; and that the crystallized is preferable as a reagent, on account of the constant uniformity of the colour it gives with iron.

2ndly, That this acid appears to be composed of acetic acid, tannin, and extractive matter; and that it cannot be completely freed from tannin by crystallization.

3dly, That the acid obtained by sublimation contains no tannin, at least that can be ascertained by acting on glue; and that it cannot, on any occasion, supply the place of the crystallized acid.

4thly, That the sublimed acid, appears likewise to be composed of acetic acid, united with a peculiar aromatic volatile oil.

5thly, That by means of water, poured into the ethereal tincture of galls, or ether containing the sublimed acid, an oily matter is separated.

6thly, That there is no process known for purifying Scheele's acid completely; that is to say, we cannot take from it the whole of its tannin, without reducing it to the state of acetic acid; which proves, that the portion of tannin it retains is necessary to constitute gallic acid, and that to this are owing its excellent properties in the art of dying.

7thly, That the red oxide of mercury, and oxide of tin, as well as carbon, decompose this acid.

8thly, That by distilling galls with water acetic acid may be obtained;

obtained; and that it is by the assistance of caloric acting more immediately on galls, that a more intimate union between the acid and the tannin is effected.

9thly, That the earthy and alkaline gallats likewise afford acetic acid by their decomposition.

10thly, and finally, That gallic acid, like the other vegetable acids, is composed of oxigen, hidrogen, and carbon.

If these results be accurate, we may conceive it possible, to accomplish its synthesis; or some trials that I have already made give me the hope of succeeding in it. I shall do myself the honour of imparting the farther results of my inquiry to the class, if they should be worthy its notice.

XI.

Observations on the Soda, Magnesia, and Lime, contained in the Water of the Ocean; shewing that they operate advantageously there by neutralizing Acids, and among others the Septic Acid, and that Sea-Water may be rendered fit for washing Clothes without the Aid of Soap. By SAMUEL L. MITCHILL, *of New York.*

(*Continued from p.* 392 of Vol. XVI.)

Observations and facts respecting the component parts contained in sea water, and the useful application of that fluid.

I FIND on experiment that carbonate of soda thrown into ocean water, immediately renders it turbid, the lime and magnesia instantly turning milky on their disengagement from their respective portions of acid. To make the water fit for washing, so much soda must be added as not only to effect a complete precipitation of these earths, but to render the water sufficiently lixivial or alkaline. It will then exert its detergent and purifying powers.

Having entertained doubts at first, whether the water ought not to be decanted of after the lime and magnesia had settled to the bottom, or whether it would not require straining or filtering to render it fit for use, I convinced myself by experiment

ment that foul linen could be rendered clean and white by being washed in alkalized ocean water which contained its whole quantity of precipitated earth diffused through it. I rather think the small quantity of those impalpable and white particles which adhere to the linen worn upon the body will be advantageous and wholesome, as the shirts and other garments will thereby be enabled to neutralize a portion of the acid and oftentimes noxious matter formed from the sweat and other excretions of the skin, &c. Thus they will be rather serviceable than otherwise, and as both are in their carbonated state (having borrowed fixed air from the soda) they cannot do any harm.

Observations and facts respecting the component parts contained in sea water, and the useful applications of that fluid.

The general inferences from the whole of the preceding reasoning are these: 1. Alkaline substances, such as magnesia and more powerfully lime and soda, are plentifully distributed through the ocean, to keep it from becoming foul, unhealthy and uninhabitable, which doubtless would be the case if the sulphuric, septic, and muriatic acids abounding in it were not neutralized. 2. Where either of these acids is but imperfectly saturated, as happens when they are united with magnesia and lime, they decompound soap, let loose its grease, and become unfit for washing by aid of that material. 3. If soda or barilla is added to ocean water in sufficient quantity and the water lixiviated or alkalized the earths will of course be precipitated and the acids neutralized. 4. In this state, dirty linen may be cleansed in it; and men at sea be thus enabled to have their clothes washed without the aid either of soap or of *fresh* water. 5. For this purpose, a quantity of barilla or soda should always be provided as an article of the ship's stores, and issued to the men on washing days. 6. Thus by the operation of this alkaline salt, a great proportion of the nastiness and infection bred in the clothes, bedding and berths of persons at sea might be prevented, and the crews and passengers so far preserved from fevers and dysenterses. 7. No more room would be occupied by water casks in the holds of vessels, than at present. 8. The small quantity of magnesia and lime adhering to clothes washed in this way, is an advantage over and above what takes place in using fresh water. And 9. A broad and noble view is opened of the economy of Providence in distributing alkaline salts and earths, so liberally throughout the terraqueous globe.

XII.

An Account of the Improvement of an extensive Tract of Land. By* RICHARD PHILLIPS, *Esq.*

SIR,

Improvement of waste land.

IN the year 1804 the waste lands in the township of Bron-y-garth, in the parish of St. Martin in Shropshire, were divided and allotted by an agreement entered into by the proprietors of land, without any application to Parliament.

This township is separated from the county of Denbigh by Offa's Dyke, the boundary in ancient times between the kingdoms of Mercia and Wales: the boundary here, as in other uncultivated parts of the demarkation, still remains entire, after a lapse of 1000 years. Upon the ground, where the improvements detailed in my paper to you are made, the descendants of the ancient Britons fought for their independence, and for what remained of their territories. Upon this spot the bands of Henry II. headed by that monarch himself, were foiled in the battle of Ceiriog by Owen Gwenydd, at the head of his brave Welshmen. The township on the west of Offa's Dyke, is called *Crogen*, i. e. *a place of graves*, because there the slain, who had fallen in battle, were buried. The posterity of the two, once hostile nations, now contend which shall excel most in the arts of peace. This rude soil is now no longer fertallized by the blood of warriors, but by the united labours of Englishmen and Welshmen. The dyke is still pretty accurately the line which separates the two languages: Welsh is generally spoken on the western side; English on the eastern. The hills, of which these wastes form a part, are at least as high as any in the county. Mr. Archdeacon Corbet, in his account of the agriculture of the county of Salop, asserts, that the hills near Oswestry are the highest in Shropshire.

The lands in question are part of the same chain which composes the skirts of the Berwyn, a mountainous tract, extending widely over the west of Denbighshire, and the contiguous part of Merionethshire. As a traveller ap-

* Society of Arts, 1796.

proaches

proaches this country from Shrewsbury, a line of highly elevated ground presents itself to his view, extending from near the Severn to the neighbourhood of Wrexham. This ground once formed the rampart of Wales, though now cultivation in several of its parts is softening the roughness of its aspect. Improvement of waste lands.

The continuity of the line of hills is broken by two principal valleys; the larger is that of Llangollen, through which the Dee flows: the other is watered by the stream of the Ceiriog. One part of the Bron-y-garth enclosure looks over the last-mentioned valley, and has a northern aspect; the other looks to the east, over the plains of Shropshire.

Lime is found in every part of the line which divides the mountains from the plains, on the frontier of North Wales. The beds of lime-stone in some places lie on sand-stone, and in other places are found below it. In others again the lime-stone is near the bottom of a hill, sand-stone occupies the middle space, and lime-stone is again found upon the summit.

In some respects the sand-stone works well, and is of a superior quality, as the aqueducts over the Dee at Pontey-sylltv, and that over the Ceiriog at Chirk, sufficiently prove. The sand-stone in the quarries, which furnished materials for building these aqueducts, is perhaps equal in beauty and durability to Bath or Portland stone; and the lime-stone, at least in one quarry near Oswestry, becomes a beautiful black marble. In the lands, spoken of below, the lime-stone supplied me with manure, and the sand-stone forms the larger portion of my fences.

The paper, which accompanies this letter, is drawn up in haste, because it was only very lately that I determined to be a candidate for the notice of your honourable Society. But all the parts are faithfully and accurately stated.

I am, Sir,
Your most obedient Servant,
RICHARD PHILLIPS.

Tyn-y-Rhos, near Oswestry,
January 1806.

To C. Taylor, M. D.

An

Improvement of waste lands.

An Account of the Improvement of more than Ninety Acres of Land lying waste.

In the year 1804 a large quantity of waste land was divided and allotted in the township where I live, on the borders of North Wales, by private agreement. I became possessed, as proprietor, of seventy acres of these lands. I obtained fifty acres more by two leases, each for twenty one years.

The wastes consisted of two divisions. The first was a piece of common land, surrounded by old enclosures. This portion, though raised far above the general level of the country, is much less elevated than the larger tract hereafter to be described.

The portion of this waste allotted to me was eight acres. The grass produced, while the land was in its natural state, was a sour rough sort. It afforded pasture in the summer to a few cattle, horses, and sheep. The coldness of the soil, and the consequent bad quality of the grass, gave this common the Welsh name of *Rhos*, a name which implies a tract of moist land, producing a coarse sour herbage.

1. I began my improvements upon this allotment, because it lay near my house. The fence is a bank four feet high from the bottom of the ditch, with a double rail at the top. A double row of quick is planted upon the top of the fence, to supply the place of the rails when they decay.

The surface soil is about six inches deep, with a substratum of bad yellow clay. The first ploughing was in June 1804. It was cross-ploughed and harrowed in August; ploughed a third time about the 20th of September; manured about the end of the same month with one thousand six hundred and ninety bushels of lime, amounting to about two hundred and eleven bushels an acre; ploughed a fourth time in the middle of October, in small butts or ridges; sown and harrowed. This operation of ridging was peculiarly necessary here to carry off the surface water, which had formerly greatly injured the land. Twenty-four bushels of Devonshire wheat were sown: the return was about two hundred and forty bushels (thirty bushels an acre). The crop was one of the finest in the county. The expences, as appear by the subjoined table, were

£. 88.

£. 88 19*s*. 1*d*. The wheat was worth last month £. 130. The balance in my favour is £. 40 0*s*. 11*d*. (£.5 2*s*. 7*d*. an acre). This land in its natural state was not worth five shillings an acre. When it is laid down in grass it will be worth 40*s*. an acre. Improvement of waste lands.

In the beginning of October 1805, the stubble was harrowed off, and conveyed to the farm-yard. The land was then ploughed, sowed with twenty-four bushels of wheat, and harrowed as the year before. This is not my usual course of crops; but it was thought that old common land could not very easily be exhausted, and I was tempted to take another crop of wheat by the high price of corn, and by the circumstance of the land being for four years tithe free. The corn now, the 12th of January, is coming up in abundance.

It is my intention to lay down this lot with grass seeds, to be sown with oats in the spring of 1807. Oats I conceive to be the best grain for the next crop, because the land is not dry enough for turnips and barley.

The second, and much larger, division of lands lying waste extended along the side, and reaches the summit of a hill, which is equal in height to any in this county. The aspect is, for the most part, north and north-east. A mountain torrent runs through the midst of this tract: some of the lands on one side of this torrent are more sheltered, and have a southern aspect.

Lime-stone is found on the lowest part of this waste, not far from the bed of a river: but the steepness of the ground above would have been too formidable an obstacle to the cultivation of the higher lands, had not lime-stone been discovered upon a spot so elevated, as to enable the improver to convey his manure, at a comparatively light expence, to the lands below.

The coals indeed, for burning the lime, are brought up a steep hill, a distance of four miles. The ascent up which they are conveyed, enhances considerably the expence of the manure.

Upon this waste the lime-stone is at the bottom of the hill, and fortunately upon the top also. The substratum, at no great distance from the surface, is sand-stone, in some places

Improvement of waste lands.

bushels of lime, the same quantity as in the last mentioned lot; ploughed a third time, and sown in the end of October, and harrowed. The quantity of wheat sown, as in the last lot, was 40 bushels. The soil is here partly sand upon the sand rock, and partly a light soil upon rammel. Hand labour was employed at a great expense upon a stony part of this lot, in quantity about three acres. The wheat plants upon this lot are of as promising an appearance as upon either of those above described.

5. The other lot of eight acres, obtained by the last mentioned lease, is not yet enclosed. The labourers are now (13th January) employed upon the fence. It was ploughed in January 1805, and harrowed in the same month. It is now a fallow intended for pease. This lot would have been prepared for wheat and sown; but lime, in sufficient quantities for all my improvements, could not this year be obtained, at the only rock from which it could be conveyed at any reasonable expense to these lands.

I was induced to offer the rents above stated of 10*s.* and 14*s.* an acre (in the last case, the land to be for seven years rent free) because I was confident, that these wastes were capable of improvement. But in the natural state in which I found them, they were not worth 2*s.* an acre. They afforded pasture to a few half-starved sheep of the worst Welsh breed; and the sheep did more damage to the fences of the old enclosed lands in winter, and to the lands themselves, than could be compensated for by the profits which their owners derived from them.

The closes now fenced and improved, are well worth a guinea an acre. A year ago they were not worth two shillings.

The improvements upon the first four lots, above described, are to a certain degree complete. They contain fifty-three acres of as fine green wheat, as any which this country contains.

6. A sixth close of thirty-two acres, allotted to me as proprietor, is fenced with a wall six feet high, and one hundred and ten roods in length: most part of the wall is pointed with mortar on the outside; the rest is to be pointed

pointed next summer. The lower part of this allotment is bounded by the fences of my old enclosed lands. On the exposed side, towards the N. W. a plantation is intended, fifty roods in length, and twelve yards in breadth. Improvement of waste lands.

This close was almost entirely covered with gorse. There was, as I stated above, much of this plant upon the lands already described. My first operation was to stock up the gorse. I gave my labourers three guineas for this work, upon this lot only. They were also to have the gorse for their own use, which was partly used for fuel, and partly sold by them. They sold it at 5*s*. the cart load. I made an experiment upon five acres of this close, where a plough could not at first be used. After the gorse was stocked off, the land was pared and burned; and the ashes were spread. The plough could, after the land had been thus treated, though with some difficulty, be used. I ploughed it in June, 1805; harrowed it; ploughed it three times more; and sowed it, about the end of the same month, with turnip seed. There is now upon the land a fair average crop of turnips.

I also pared three acres more. Part of this was burned, and part was manured with dung. Where the dung was laid, the ground was trenched about nine inches deep; the sod was placed with the surface downwards within the trench. The dung was laid in moderate quantities upon the sod, and covered with about six inches of soil. Potatoes were then set in the beginning of May in rows. They were hoed twice. The produce was abundant.

The remaining 24 acres of this inclosure, were ploughed in February and March, 1805. Fourteen acres were, after one ploughing, sown with 71 bushels of black oats; 11 acres with pease; and three acres with summer vetches. The ground was then well harrowed. I had little land of my old enclosures this year in oats. It was my wish to try, whether a crop of this grain might be obtained, upon land so fresh and light as this, without manure, and with one ploughing. The first promise, however, of the oats, was so bad, when they began to appear above the ground, that I thought it best to throw some lime upon the land; which, if not so beneficial to the crop of oats, will be useful

Improvement of waste lands.

ful to any succeeding crop. I therefore manured 18 acres: viz. all the land where the oats were sown, and part of that sown with pease, with 4550 bushels of lime.

I obtained from the 71 bushels of oats, a return of, 360 bushels; a clean, thin crop, intermixed, indeed, with a little fern. The pease and vetches produced but a poor return.

In the beginning of November my teams were not much employed. I sent them to try how this land would appear when ploughed up. I found the part which had been limed remarkably mellow. I conceive that this favourable appearance, arose from the length of time that the lime had been upon the ground. I then procured several hired teams in addition to my own. It was all ploughed up by the twentieth of the same month; sown with 95 bushels of wheat, and harrowed. The potatoe land was sown with wheat, at the same time. The six acres, which had not been limed, are to be manured with 240 bushels of soot. The soot is now in waggons upon the ground; and the first favourable day, it will be thrown upon the land as a top dressing.

This is the last field sown by me. The wheat plants are now (January 13) making their appearance above ground, and look well.

I intend this year to proceed with activity in the improvement of the following allotments, which still lie waste.

7. and 8. One of these lots of 25 acres I obtained in exchange for four acres and a half of old enclosed, arable land, detached from my farm, of much the same quality with other arable lands in this neighbourhood. This circumstance alone proved of how little value these waste lands were. These 25 acres were an object to me, as they lay contiguous to another of my allotments; and they are equal in goodness of soil to any upon this hill. After this land has undergone the process described in lots 2d and 3d, I hope to see these 25 acres of equal value, acre by acre, to the four and a half which I gave for them.

These 25 acres, as well as lot 8, 27 acres, allotted to me as proprietor, will be improved next summer.

9. Three

9. Three acres of steep ground, that can never be cultivated, will be planted, this spring, with different kinds of forest trees. Improvement of waste lands.

The tables of expenditure are below. The return in the first lot has more than repaid all my expences the first year; and the return promised by the three succeeding lots, is little less abundant.

If I am permitted to live another year, and to enjoy my usual health, I hope to see 148 acres of land, which was so lately almost entirely unproductive, covered with golden harvests, or adorned with thriving plantations.

LOT I.—8 *Acres.*

	1804.	£.	s.	d.
64	roods of fencing, at 7*s.* per rood - -	22	8	0
1	gate and posts - - - - -	1	1	0
1690	Winchester bushels of lime, at 2½*d.* per bu.	17	12	1
	Carriage - - - - - -	5	4	0
	1st ploughing, 20*s.* per acre - -	8	0	0
	2d ditto 15*s.* per do. - -	6	0	0
	3d ditto 10*s.* per do. - -	4	0	0
	4th ditto 10*s.* per do. - -	4	0	0
3	separate harrowings, at 15*s.* per acre—5 acres	6	0	0
24	Winchester bushels of wheat, at 11*s.* do.	13	4	0
	Labour - - - - - - -	1	10	0
	1805.			
1	ploughing and harrowing, at 15*s.* per acre	6	0	0
24	Winchester bushels of wheat, at 11*s.* -	13	4	0

LOT II.—21 *Acres, Fenced by Landlord.*

85½	roods of stone walling, at 17*s.* per rood	72	13	4
3	gates, at 1*l.* 1*s.* each - - - -	3	3	0
5200	Winchester bushels of lime, at 2½*d.* per bus.	54	3	4
	Carriage - - - - - -	10	0	0
	1st ploughing at 20*s.* per acre - -	21	0	0
	2d ditto at 15*s.* per do. - -	15	15	0
	3d ditto at 10*s.* per do. - -	10	10	0
3	separate harrowings, at 15*s.* per do. -	15	15	0
71	Winchester bushels of wheat, at 11*s.* per b.	39	1	0
	Labourers for stocking, levelling, clearing stones, and spreading lime - -	15	5	2

LOT

Improvement of waste lands.

LOT III.—12 *Acres*, *Fenced by Landlord.*

26½	roods of stone walling, 6 feet high, at 17*s.* per rood - - - -	22	10	6
67½	roods of fencing, with stone face, and double posts and rails, and hawthorn quick, at 10*s.* per rood - - -	33	15	0
58	roods of sunk fence, 5 feet deep, and staggers on the top along the dyke, at 4*s.* 9*d.* per rood - - - -	13	19	6
2	gates and posts, at 1*l.* 1*s.* each - -	2	2	0
	1st ploughing, at 20*s.* per acre - -	12	0	0
	2d ditto at 15*s.* per do. - -	9	0	0
	3d ditto at 10*s.* per do. - -	6	0	0
3	separate harrowings, at 15*s.* per acre -	9	0	0
3250	Winchester bushels of lime, at 2½*d.* per bushel - - - - - -	33	17	1
	Carriage - - - - - -	6	5	0
40	Winchester bushels of wheat, at 11*s.* per bushel - - - - - -	22	0	0
	Labourers, for stocking, levelling, and spreading lime - - - -	8	3	0

LOT IV.—12 *Acres*, *Fenced by Tenant.*

60	roods of stone walling, at 15*s.* per rood	45	0	0
30	ditto of fencing, with single posts and rails on top, with hawthorn quick, at 9*s.* 4*d.* per rood - - - -	14	0	0
2	gates and posts, at 1*l.* 1*s.* - - -	2	2	0
	1st ploughing, at 20*s.* per acre - -	12	0	0
	2d ditto at 15*s.* per do. - -	9	0	0
	3d ditto at 10*s.* per do. - -	6	0	0
3	separate harrowings, at 15*s.* per do. -	9	0	0
3250	Winchester bushels of lime, at 2½*d.* per bushel - - - - - -	33	17	1
	Carriage - - - - - -	6	5	0
40	Winchester bushels of wheat, at 11*s.* per bushel - - - - - -	22	0	0
	Labourers, stocking, levelling, clearing, and spreading lime - - - -	14	8	6

LOT

LOT VI.—32 *Acres.*

Improvement of waste lands.

110	roods of stone walling, 6 feet high, at 17*s.* per rood	93	10	0
6	ditto of fencing, at 9*s.* 4*d.* per rood	2	16	0
3	gates and posts, at 1*l.* 1*s.* each	3	3	0
	1st ploughing, at 20*s.* per acre	32	0	0
	2d ditto at 15*s.* per do.	24	0	0
2	separate harrowings, at 10*s.* per acre	16	0	0
4550	Winchester bushels of lime, at 2½*d.* per bushel	47	7	11
	Carriage	8	15	0
71	bushels of oats, at 3*s.* 4*d.* per bushel	12	7	0
24	ditto of pease, at 5*s.* per bushel	6	0	0
8	ditto of summer vetches, at 6*s.* per bushel	2	8	0
240	ditto of soot, at 6*d.* per bushel	6	0	0
	Sowing the same, at 5*s.* per 100 bushels	0	12	0
95	Winchester bushels of wheat, at 11*s.* per bushel	52	5	0
	Paring and burning 5 acres, for turnips, at 1*l.* 16*s.* per acre	9	0	0
2	extra ploughings, at 20*s.* per acre	5	0	0
2	ditto harrowings, at 10*s.* per do.	2	10	0
	Spreading the ashes, at 2*s.* 6*d.* per do.	0	12	6
5	pounds of turnip seed, at 1*s.* per pound	0	5	0
2	hoeings, at 7*s.* per acre	1	15	0
150	roods of 8 yards square of stocking, at 1*s.* per rood, for potatoes	7	10	0
	Paring 3 acres, at 1*l.* 6*s.* per acre	3	18	0
	Labourers for clearing the gorse	3	3	0
	Ditto levelling and spreading the lime	5	15	0

LOT V.—8 *Acres, a Fallow for Pease; One Acre Waste.*

	1st ploughing, at 20*s.* per acre	7	0	0
	Harrowing, at 5*s.* per do.	1	15	0
	Total amount £.	1073	1	0

SCIENTIFIC

SCIENTIFIC NEWS.

On the Tempest of Feb. 18, *which has produced many dreadful accidents in the Channel.*

Observations by La Marck on his system concerning the influence of the moon upon the weather.

THE interesting nature of the observations I am about to communicate, appears to me to be of too serious an importance to permit any consideration to delay their publication, to which I wish to give the greatest authenticity.

I have been long convinced by observation, that many points in the course of the moon have unquestionable influence on the atmosphere, although the causes which modify these influences are not sufficiently appreciated to enable us to predict what events may be expected at those periods.

I add, that the results of my observations, recently completed, has strongly confirmed my opinion in this respect, and has informed me, that, independently of the influences of the *syzigies*, the *quadratures*, and the two *apsides* the *nodes* of the moon have very remarkable influence, but more powerful in some particular cases, as I have succeeded in ascertaining.

Of 311 *nodes* and *contra-nodes* marked in my collecton of Observations, 177 have eminently distinguished their influence; 134 have manifested no particular power. The difference is 43 in favour of the influence of these lunar points. But I observe that the *contra-nodes* have somewhat more power than the *nodes*, and that especially the power of those *contra-nodes* which occur during the half-yearly period of the sun's being north of the line, deserve the most serious attention. There are even circumstances wherein I find that the evil influence of the *contra-nodes* has never failed to shew itself. I shall describe them, as well as the details of my recorded observations, in the next *Annuare Météorologique*.

But it is of consequence that I should explain to the public, that the tempest of the 18th of February last is the result of a *contra-node* which took place the evening before, under circumstances which I promise to explain.

(To be continued.)

A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY AND THE ARTS.

JULY, 1807.

ARTICLE I.

Facts toward a History of Prussiates. *By* Mr. PROUST.*

THE Prussian blue of the shops is seldom pure, as Scheele had already observed. Frequently, beside the alumine which makes a part of it, we find silex, carbonate and sulphate of lime, sulphate of potash, phosphate and red oxide of iron, sulphur, ammonia not divested of animal oil, &c. To study the nature of this combination therefore, it is indispensable, to use only a prussiate free from alum, and sufficiently edulcorated with acids and boiling water.— It even appears, according to a remark of Berthollet, that the prussiate of potash can attach itself to the Prussian blue so forcibly as to resist ablution to a certain degree. I do not think with him however, that the surcharge of this salt should be considered as an element essential to it; for when the blue has been well prepared, and such is to be met with in the shops, it leaves no trace of saline matter in the residuum after distillation.

Impurities of Prussian blue.

Prussiate of potash not essential to it, as Berthollet supposed.

Prussian blue prepared without alum has a coppery appearance like the best indigo. It loses only forty-five *per cent* by combustion. Its residuum is red oxide of iron without any mixture of foreign matter.

Pure Prussian blue.

* Annales de Chimie, Vol. LX. p. 185. Nov. 1806.

Action of Alkalis.

Treated with caustic potash,

The blue passed through caustic potash leaves a residuum, which is nothing but red oxide blended with alumine. Its colour is that of kermes, if the blue were of good quality; on the contrary it is pale and earthy, if the blue were surcharged with alumine: so that we may form a pretty good judgment of its nature by the colour of its residuum.

may be deprived of all its acid at once.

Acids acquire no colour from the residuum properly washed; which shows, that Prussian blue may be deprived of all its acid at a single operation; but for this it must have been very finely pulverized, which is attended with some difficulty. If a few drops of alkali be added to water coloured by the blue recently precipitated, it will be deprived of its colour completely; and in this case the oxide separated from it will not afford the least trace of colour, when it is wetted with an acid.

This seldom the case.

In the process followed it frequently happens that the ochry residuum retains either some remains of blue, that have not been touched by the alkali, or a mixture of prussiate of potash and ferruginous alkaline carbonate, or all three of these blended together. I shall proceed to examine two of these cases, and it will be easy to form a judgment of the third.

Blue left in the residuum.

If for example an acid be applied to a well washed residuum, which still retains Prussian blue, this blue will not discover itself in pulverulent particles, but in proportion as the acid frees it from yellow oxide. There is no particular chemical union between this oxide and Prussian blue, as hitherto has been supposed; at least we have no positive indication, that the metallic salt, called prussiate of iron, is susceptible, like so many others, of a *maximum* and *minimum* state either of acid or of oxide; and if the mixture of yellow and blue, which these residuums sometimes offer us, be not green, as might be expected, it is because the yellow oxide always covers these remains of blue in very great excess; at least I have never found the blue to be above one or two hundredth parts.

Prussiate of potash, and ferruginous alkaline carbonate,

I proceed to the second case. A residuum may contain no remains of blue, if it were well pulverized, but it easily retains the two salts I have mentioned above. If an acid be then applied to it, each of them affords abundance of

blue. We shall examine the particular mixture of these two salts farther on: but if the residuum have been washed with care, acids will not give rise to more blue. This washing is very tedious, it is true; for I have been obliged to pour boiling water at least twenty times following on a single drachm of residuum, before I could obtain it completely free; but when this is at length accomplished, acids will dissolve it, without any blue being produced.

not easily removed by washing.

When these residuums effervesce with acids, they contain carbonate of potash or of lime. The former may be carried off by ablution; and the second will be detected by vinegar after the washing. It is not the red oxide therefore, that occasions this effervescence: it cannot indeed combine with the carbonic acid, consequently cannot take it from the potash, in exchange for the prussic acid, which it cedes to it. In nature, as in art, it is only the oxide of iron at a minimum, that can combine with the carbonic acid.

Effervescence indicates a carbonate.

Red oxide of iron does not unite with carbonic acid.

A pound of Prussian blue of the shops of fine quality, has afforded as much as nine ounces and half of crystallized prussiate of potash. It is by no means uncommon, to find in the mother water left to itself truncated octaedra an inch in diameter. If the blue be contaminated with sulphuric acid, at least four crystallizations will be requisite, to free the prussiate entirely from sulphate of potash. The mother water contains alumine, sometimes in abundance, sulphate and phosphate of potash, ferruginous alkaline carbonate, &c. Hence may be inferred the importance of employing crystallized prussiate of potash in analyses, and not simple lixivia of Prussian blue, as was formerly done. Prussiate of potash is unalterable in the air, whether dry or damp; boiling it for any length of time does not alter its nature: its taste is sweetish, and slightly saline, leaving after it a faint impression of bitterness. It is insoluble in alcohol; which separates it from its aqueous solution in a white snow with the lustre of mother-of-pearl; and this lustre it retains when dry, so that it might be mistaken for the argentine gauze of acetate of mercury. Redissolved in water it reproduces a common solution of triple prussiate.

Prussian blue affords $\frac{1}{32}$ of prussiate of potash.

Lixivia not pure,

the crystals therefore should be used as a test.

Characters of the prussiate,

This salt, which I shall term a triple salt, or trisule, indiscriminately, to distinguish it from the simple prussiate of

which is a triple salt,

potash, is as constant in its qualities as the most perfect neutral salts. It is of a fine lemon colour, which it never loses without changing its state. For this, as well as for its other two characteristic properties, that of crystallizing, and of changing the red oxide of iron blue, it is indebted to a portion of black oxide, which is essential to its constitution. Without this oxide, confined like the other two elements of the triple prussiate to an invariable proportion, this prussiate in fact could neither crystallize, nor form blue with solutions of iron, the base of which is at a maximum of oxidation. In short, it is from this very union, that the principle which saturates the potash of the triple salt derives those properties, as Berthollet remarks, that singularly increase the analogies it bears to acids.

Containing black oxide of iron.

This gives the prussic acid more decidedly acid properties.

In this point of view we may add, that the triple prussiate occupies a mean betwixt alkaline and metallic salts.—However, when we reflect on one property of this salt, which will be mentioned below, it is difficult to say, whether it be to the prussic acid simply, or to the combination of this acid and potash, that the oxide of iron attaches itself, when it converts the prussiate into a triple salt. Thus much is certain, that we do not yet by any means know what appearance or properties a prussic acid might have, that should be combined with the precise dose of black oxide, by means of which it can furnish a triple prussiate. By treating this oxide with prussic acid we can form Prussian blue, but not that kind of ferruginous acid, which is capable of converting potash into a triple salt. Of this we must not lose sight; for it is well known, that Prussian blue is not of a nature to combine with potash without leaving a residuum. In short, the triple prussiate divested of its alkaline base, if I may so say, is a compound, which no fact, no appearance authorizes us to consider rather as a salt, the acid of which has been particularly exalted by its union with the oxide, than as a combination perfected altogether by this oxide.

This combination of black oxide and prussic acid not obtainable by itself.

Apparently a joint affinity between the acid, oxide, and alkali.

One property, which appears in fact to militate against our admitting the prussiate as a salt, the acid of which is exclusively united to the black oxide, is that of its resisting the action of alkaline hidrosulphurets. If these reagents, which

which spare no other known metallic salt, have no action on the triple prussiate, we are to a certain degree justified in presuming, that the oxide of iron is not exclusively attached to the acid of the triple prussiate; unless indeed we suppose, that the affinity of this acid for the oxide be so great, as to defend it from the common fate of all other oxides. We shall see however farther on, that an affinity so extraordinary, unexampled as it has hitherto been in chemistry, is not impossible. I proceed to the trial of the hidrosulphuret of potash on the triple prussiate.

Hidrosulphuret and Triple Prussiate.

The hidrosulphuret of potash or of ammonia, even assisted by heat, has no action on this salt. If it contained any remains of ferruginous carbonate, it would be freed from them, for the hidrosulphuret decomposes this carbonate. It may be filtered, if necessary, and the prussiate will nevertheless crystallize in its usual form. Such a result leads us to acknowledge, as has been hinted above, a very peculiar intimate combination between the three elements of the triple prussiate. But we shall presently see these very hidrosulphurets contribute to our obtaining the white prussiate in all its purity, or that combination in which the iron is at a minimum state of oxidation, which I made known in my former memoir on Prussian blue.

The triple prussiate not decomposed by hidrosulphurets.

White Prussiate.

Over a lamp place a matrass containing fifteen or eighteen grains of prussiate of potash, and two or three ounces of hidrosulphuretted water. A few seconds after the ebullition and vapour have expelled the air, that occupied the upper part of the matrass, drop in slowly a very dilute solution of green sulphate of iron from a phial, into which a few grains of sulphuret of the same metal have previously been put, in order to keep its base at a minimum of oxidation. Immediately a precipitate will be formed, rendering the liquor as white as milk, and so it will remain as long as the heat is kept up. This is the precipitate which I call white prussiate, and is the same as has been obtained by Fourcroy, Vauquelin, Davy, and no doubt all, who, paying

White prussiate of iron.

ing attention to the conditions necessary to ensure success, have found, that the base of the green sulphate might become that of a prussiate different from the prussiate which has for its base oxide at a maximum. But as the black oxide, in passing from one combination to another, never loses its disposition to acquire a surplus of oxigen, we perceive, as soon as the matrass is removed from the fire, that the atmosphere reacts on the milky mixture, and rapidly produces tints of colour, which gradually diffuse over the whole a fine deep blue.

Rendered blue by oxigen from the atmosphere.

Another method of obtaining white prussiate.

This product may be obtained in another way. Let fall prussiate of potash grain by grain into a very dilute boiling solution of green sulphate, and a precipitate will make its appearance, the whiteness of which will resist the action of the air somewhat longer.

I shall add a few other processes, which, if they do not increase conviction, may be interesting from the variety of the means employed.

A third.

Fill two glasses, one with nitrate of iron, and the other with green sulphate, each in a state of very dilute solution; and drop into each a crystal of prussiate of potash. In the former we shall see the crystal instantly coloured of so deep a blue, that it resembles black velvet. In the latter it cracks, separates, and falls into a white powder: but, as it had imbibed atmospheric air previous to the experiment, the precipitate is variegated like sage cheese.

A fourth.

Let two glasses be filled with boiling water; add to each a few drops of prussiate of potash, and to one of them a few drops of hidrosulphuret of potash, or of ammonia, likewise. Let fall into each a few drops of nitrate of iron; and the former, as might be expected, will give a complete blue; but the latter will exhibit the amusing appearance of a precipitate, which, at first blue, will rapidly lose its colour, and become white. The theory of these facts is so obvious, that I shall pass it over: neither shall I here repeat all the other experiments given in my first paper to establish the existence of two prussiates of iron. If the prussiate at a minimum have no colour when it is not acted upon by the atmosphere, we see, that the dried green sulphate is equally colourless. The absence of colour in one of these salts is surely

White prus-

surely not more surprising than in the other; and, if we obtain red oxide by applying alkalis to a blue prussiate, on the contrary we obtain black oxide from a white prussiate. But these differences, which might be inferred from theory, agree perfectly with those exhibited by the red and green sulphates under similar circumstances.

siate gives black oxide on being precipitated.

In my first paper I directed to pour the prussiate of potash on the sulphate in a phial, to avoid as much as possible the admixture of air; but this succeeded imperfectly; first because cold liquors always have air in them; and secondly because I had not thought of sulphuretted hidrogen to free them from it. I did not then know how it acted with respect to these salts.

Caution.

If for instance a solution of green sulphate be diluted with three or four times its bulk of sulphuric or muriatic acid, the excess of these acids makes no alteration in the result. As the white prussiate wants colour only from defect of oxigen, it may be supposed such an addition is not calculated to impart it. These concentrated acids may alter the whiteness of the prussiate indeed, but they can never bring it to a complete blue.

Excess of acids do not change the white prussiate to the blue as they do not supply the necessary oxigen.

Muriatic acid boiled on white prussiate is equally ineffectual.

Not that this boiling acid is without action on the white prussiate; for I have observed, that there is some white prussiate destroyed, prussic gas evolved, and black oxide found in solution. The little Prussian blue, that is formed by the introduction of air, during the interval of these mixtures, predominates over the white, and changes its colour to a greenish.

Action of muriatic acid on it,

The blue prussiate boiled with the same acid likewise gives out prussic gas, and parts with red oxide, but less of it is destroyed than of the white prussiate. From these facts we may infer, that the muriatic acid, assisted by heat, is capable, in strictness, of decomposing prussiates, and assuming its rights of a stronger acid over the prussic; which would not be at all surprising, but at least I believe it would require considerable time.

and on the blue prussiate.

Prussiate

Prussiate of Potash and Acids.

The crystallized prussiate treated with acids.

Let weak sulphuric acid or muriatic be heated in a matrass with crystals of prussiate of potash. When ebullition commences, gas escapes, and may be received under a jar over mercury, or burned by applying to it a lighted candle. The flame will be variegated with red, violet, and yellow; and during the extrication of the gas the liquor will be thickened by the production of a white precipitate, which changes to a blueish. When all the gas is evolved, throw the mixture into boiling water, brighten it with oxigenized muriatic acid, wash, and dry the product in a capsule. Four experiments, made at different times, afforded me thirty-four or thirty-five parts of complete blue from a hundred of the triple prussiate.

It affords ·34 or ·35 of Prussian blue.

I proceed to the consequences:

Prussian blue contains ·54 or ·55 of red oxide of iron.

A hundred parts of Prussian blue, without alum, yield fifty-five of red oxide by combustion. The same blue, destroyed by nitric acid, gives fifty-four. It cannot be questioned therefore, that Prussian blue contains fifty-four or fifty-five hundredths of red oxide. From these data thirty-five parts of blue ought to produce about seventeen of black oxide, or nineteen of red. Hence it follows, that, when iron was formerly separated from a solution by prussiate of potash, this salt added to the product the nineteen hundredths of red oxide arising from its own decomposition; but the addition was still greater, when a simple alkaline lixivium of Prussian blue was used instead of the crystallized prussiate. The reason of this we shall see presently.

The prussiate therefore, used as a test, adds 19 per cent to the oxide thrown down.

Prussian blue treated with common potash.

When Prussian blue is treated with a common lixivium of potash, part of the alkaline carbonate loads itself with red oxide; and the result is a solution answering to Stahl's martial tincture, of which pure potash is insusceptible.—This solution, which may be prepared likewise by adding a few drops of nitrate of iron to a solution of carbonate of potash, will not occasion the least change in prussiate of potash, even by standing together. It is the same ferruginous carbonate, which, as I have said, is found in the mother-water. In effect, if an acid be added to the mixture of these salts, a perfect blue is precipitated, because the

new

new solution of oxide, which takes the place of the ferruginous carbonate, decomposes the prussiate of potash, as any solution of iron would do.*

When Prussian lixivium therefore is employed instead of a crystallized prussiate in an analysis, we add to the product in the first place red oxide, which made part of the ferruginous carbonate; and in the next black oxide, which is a constant element of the triple prussiate contained in the lixivium. Inaccuracy of the Prussian lixivium as a test.

Chemists very soon discovered the faults of these lixiviums, though they were not at first aware, that they contained two very different combinations of iron, the carbonate of which I am speaking, and the triple prussiate. Many, on seeing the blue they yielded with acids, even thought this blue existed in them as a distinct substance; and endeavoured to precipitate it, whether it were Prussian blue or oxide, not affecting the alkaline prussiate, which they supposed to possess the tinging property without being indebted for it to iron. From their attempts arose the receipts for precipitated lixiviums, which occur in every work on chemistry. But since the inquiries of Scheele and Berthollet, it has been found, that these receipts answer the end but imperfectly; for it is easy to see, that it was not sufficient to free a lixivium from the oxide introduced into it by the carbonate; there remained farther to be guarded against the black oxide, which belongs to the triple prussiate, and the existence of which was the less suspected, because the addition of acids, without the intervention of light or heat, could not render the products of its decomposition perceptible. Attempts to correct it.

I shall not stop to analyse the phenomena, that presented themselves during the preparation of lixivia either hot or cold, because, as the inutility of prussiates for the evaluation of iron in analyses is now well known, the particulars would not be very interesting. In the same manner I shall pass over the proposed test liquors with ammonia, lime, magnesia, &c. because they are themselves triple prussiates, Other prussiates as inaccurate, without a counterproof.

* It is the mixture of these same salts, which enables the mother-waters of soda to afford Prussian blue by the addition of an acid. Prussian blue from mother water of soda.

on

on which consequently we cannot rely, unless we join with them the counterproof proposed by Berthollet. I shall only add, because this should remain consigned to the history of the science, that, when a chemist still employs with effect a lixivium, or test liquor, purified by an acid, we may be certain, he has not obtained a complete and entire separation of the iron, as he flatters himself: for it is certain, that every lixivium capable of affording blue with a solution of red oxide contains black oxide, since without the assistance of this oxide there would be no tingeing prussiate; in other words, every prussiate of potash, not rendered a triple salt by black oxide, is incapable of forming blue with a solution of iron, the oxide of which is at a maximum, as is most commonly the case with those produced in analyses. This is a fact which Scheele perfectly developed. We may conclude therefore, that the alkaline or earthy triple prussiates can no longer be considered as useful in analysis, any farther than litmus, galls, and other reagents, which merely indicate the presence of a certain principle, without being capable of ascertaining its quantity.

No prussiate will produce Prussian blue, unless black oxide of iron be present.

Prussiates of no farther use than to indicate the presence of iron.

Action of sulphuric acid.

The aqueous sulphuric acid, applied to the triple prussiate, affords the same results as the muriatic. A hundred parts of prussiate produce a hundred and fifteen or a hundred and sixteen of sulphate of potash. If we knew exactly how much alkali the sulphate contained, we might thence deduce the quantity of the base of the prussiate. A hundred parts of crystals of prussiate lose ten of water by distillation.

Boiling necessary,

To complete its decomposition by the acids, it must be kept boiling at least half an hour, in order to dissipate the gas entirely, and obtain the entire separation of the white prussiate, which is formed during the operation.

or the assistance of light.

The prussiate of potash dissolves cold in the muriatic acid, without being decomposed. This mixture, as Berthollet found, requires the assistance either of light, or of heat.

Action of vinegar.

Vinegar, assisted by boiling, decomposes it also: prussic gas escapes, and white prussiate is formed, which does not change blue so quickly as with the preceding acids: finally, this prussiate, which does not appear till the moment

ment when the ternary combination begins to dissolve, confirms by its whiteness the fact, that it is only the oxide at a minimum, which has the privilege of entering into the formation of the triple prussiate. This is one of those truths, on which Scheele left nothing to be desired; yet the distinction of the oxides in this substance is a point, to which subsequent chemists have not paid all the attention it deserves.

The black oxide of iron only forms a tiple prussiate.

Black Oxide, an Element of Prussian Blue.

We have just shown, that this oxide, in a constant proportion, is essential to the constitution of the triple prussiate; but there is another object, that has also some claim to attention, which is, that this oxide is capable of following the prussic acid from one combination to another, without changing its state; that it can pass from prussiate to prussiate and back again, and even circulate through the most oxiding mediums, without losing the state of a minimum oxide: and this I conceive to be a point of view, which has been overlooked in the history of prussiates.

It is one essential principle of prussian blue.

If, for instance, we may say with truth, that the prussiate of potash would be neither yellow, nor crystallizable, nor tingeing; we may assert with equal foundation, that neither would prussian blue be formed without the intervention of this oxide: and in fact, when we make prussian blue with a solution of red oxide and of triple prussiate of potash, the black oxide in the latter salt enters into the new combination jointly with its acid; whence it follows, that this oxide, which is an element of the triple prussiate of potash, becomes so afterward of prussian blue; and even, as will be seen presently, of all the other metallic prussiates, that are made with this salt.

This black oxide is so firmly intermixed in the compound of prussian blue, and so well defended from all farther oxigenation by its union with the prussic acid, that we never fail to find it again in this blue such as it was in the triple prussiate of potash. I will say more; if we make the blue with this prussiate and the green sulphate of iron, the oxide of the latter will be raised, as is well known, to its maximum, in proportion as the blue becomes coloured by

Resists farther oxigenation when thus combined.

by the impression of the air; but it certainly will not be the same with the black oxide, which passes into the prussian blue jointly with the acid. This oxide will not lose the quality of being at a minimum, which it had in the prussiate of potash; that is to say, if, during the exposure to air, the basis of the green sulphate, and consequently that of the white prussiate, raise its proportion of oxigen from 28 to 48 per cent, the black oxide, the inseparable companion of the prussic acid, will not participate in this super-oxidation, but will invariably keep to its 28 per cent.

Neither the air, nor boiling nitric acid, nor oxigenized muriatic, oxide it more, except as these acids destroy the blue itself.

Not only the atmosphere, which so easily raises the bases of the sulphat, muriate, and white prussiate to their maximum, loses all its activity when applied to the black oxide in question; but neither boiling nitric acid, nor the oxigenized muriatic, can increase its oxidation. These acids are capable indeed of destroying prussian blue, and even reducing it to red oxide; but as long as any blue remains to be destroyed, this will to the last retain its black oxide in all its primitive integrity.

Red oxide of iron will not combine with prussic acid, but black will.

If red oxide be treated with prussic acid, no kind of combination will take place between them. This is agreeable to the observation of Scheele. But if we employ black oxide, we shall obtain a greenish prussiate, which will be rendered perfectly blue by the action of the air. Black oxide therefore enters into the composition of prussian blue. If this oxide were unnecessary, or if the red oxide might serve exclusively as the base of prussian blue, it does not appear why this oxide, brought into contact with the prussic acid, and even its solution mixed with simple prussiate of potash, should not afford prussian blue.

Affinity of the prussic acid for a given proportion of black oxide, very strong.

I have remarked above, that the affinity of the prussic acid for such a dose of black oxide, as adapts it to the production of the triple prussiate, may be sufficiently powerful, to protect it from the common fate of oxides combined with acids in general: and in fact it appears to me, that this inference may be drawn from the following experiments.

Experiment 1. Pour hidrosulphuret of potash into a phial on prussian blue

blue, and keep the mixture closely stopped: at the expiration of a few days, the hidrosulphuret will be converted into a triple sulphate, and the red oxide of the prussian blue only changed into black hidrosulphuret. Whence it appears, that, if the red oxide have followed the example of all other oxides, when the hidrosulphuret finds them combined with acids, it is not the same with the black oxide.

Experiment 2. Hidrosulphuretted water brings back prussian blue to the state of white prussiate, as it does the red sulphate to that of green sulphate. This is a fact which I made known in my first memoir, and the power of this reagent never goes farther; but the hidrosulphuret of potash completely changes the red and green sulphates into black hidrosulphuretted oxide. Why then cannot this hidrosulphuret extend its action to the black oxide in question? Certainly some singular affinity, of which I believe there are few instances in chemistry, enables the prussic acid, the weakest of all acids in so many respects, to protect this oxide against all the power of the alkaline hidrosulphurets.

The same in other prussiates. All the metallic solutions, that afford prussiates with the triple prussiate of potash, no doubt follow the example of those of iron. The prussiates resulting from it will retain in all its integrity the black oxide, which the prussic acid carries with it; but it is time to lay before the reader the capital experiment, which demonstrates, that prussian blue is a triple salt; and that the black oxide, which had passed from the triple prussiate of potash into the prussian blue, is capable of passing back again from the prussian blue to potash, without having for a moment quitted its state of a minimum oxide. This experiment I have no doubt is anticipated by every one, who has formed a clear idea of the triple prussiate of potash.

Proof that prussian blue is a triple salt. Let us take, for instance, a prussian blue, which has experienced all the action that the atmosphere, or the most oxiding acids, can exert upon it. Let us apply to it pure potash, and we shall obtain a lixivium, which will yield only a triple prussiate, or that combination in which we find the prussic acid constantly united with the usual dose of black oxide. If this prussiate be really such as I have announced, and the reader will have no difficulty to believe,

there

there can be no objection I imagine to the theory that asserts, that the white or blue prussiates are triple combinations, as well as the prussiate of potash, which has concurred to form them.

Triple prussiate of manganese, and of copper.

Prussiate of manganese being put into a solution of potash, the result was the crystallizable triple prussiate of potash, of a yellow colour, and containing its due proportion of black oxide. This prussiate of manganese then is a triple combination, containing the black oxide. The prussiate of copper of a sanguineous colour is no doubt another, for the simple prussiate of copper is yellow.

Perhaps other prussian pigments.

Scheele informs us, that other oxides also have the property of converting the simple prussiate of potash into a triple salt. This apparently opens a field to a series of researches, which are the more interesting, as they may lead to the discovery of some colour equally valuable with prussian blue; and lastly we may conclude, from all that has been said, that no simple prussiate of iron exists; a kind of combination however, of which other metals are susceptible, as will soon appear.

Simple prussiates of other metals.

Distillation of Prussian Blue.

Destructive distillation of prussian blue.

This prussiate is destroyed by exposure to a high temperature. The new products, that arise from it, confirm the theory Berthollet has given us respecting the nature of the prussic acid. We obtain an acid which escapes destruction, carbonate of ammonia, a little free carbonic acid, gaseous oxide in abundance. An ounce of good blue of the shops afforded rather more than five pints of this gas, with as much carbonic acid as made up the whole three quarts. The water of the trough contained prussic acid fixed by ammonia. This prussiate, as is well known, follows the steps of that of simple potash; it cannot produce blue with solutions of red oxide, but it does with those of oxides at a minimum, because at the same time it forms itself into a triple or tingeing prussiate.

A pyrophorus.

The residuum weighed five drachms fifty-two grains. It was perfectly black, and very attractable by the magnet. It is a pyrophorus, which takes fire with rapidity. After it has been kept in a phial not closely stopped, so long that it will

will not kindle of itself, if it be wetted with nitric acid of 40°, it burns with great vividness. I am inclined to think, that in this combustion the iron burns in conjunction with the charcoal.

If the prussian blue were without alum, this residuum contains nothing but charcoal and iron. Residuum.

Muriatic acid disengages from it with the greatest facility that aromatic hidrogen, which announces iron steelified, or combined with carbon. The residuum is pure carbon, one of the elements of the acid destroyed. As to the carbonic acid and gaseous oxide, it is equally evident, that they are the two oxidations of carbon, a maximum and minimum, produced by the oxigen of the two oxides found in the prussian blue. Treated with muriatic acid.

This decomposition is obtained by a heat so gentle, that it appears to me a convenient mode of procuring the gaseous oxide of carbon. As there is not the slightest appearance of oil, it is somewhat surprising, that in the course of the destruction of a compound in which carbon and hidrogen abound, no part of these combustibles should be found to present themselves under circumstances in which oil would be formed. Gaseous oxide of carbon. No appearance of oil.

The oily and aromatic character, that the hidrogen assumes during the solution of the residuum, demonstrates likewise, that the combination of iron with carbon does not require a very high temperature. The charcoal of blood, which is obtained by a very low heat, equally contains iron in the state of carburet; for this likewise yields odoriferous hidrogen with muriatic acid. I think I have somewhere else observed, that Priestley was struck with the bituminous smell of the hidrogen furnished by carburetted iron. Iron unites with carbon at a moderate heat.

Distillation of the Triple Prussiate of Potash.

This salt loses ten per cent of water, and with it its colour, for it becomes white; but it does not begin to soften till it is at a red heat. Some chemists have imagined, that roasting or melting it would afford the means of freeing it from oxide, but the following results will show, that these processes lead to nothing useful. Destructive distillation of the triple prussiate of potash.

When

Progress of its decomposition by heat.

When this salt enters into fusion, a little prussic acid escapes, which is seized by the ammonia that is formed at the same time. Afterward a nebulous vapour rises, which condenses in the neck of the retort in a meally powder. This vapour is not reproduced when the fusion is at an end. On examination the sublimate has the alkaline and bitter taste of simple prussiate.

Alcohol dissolves a portion of it, and what separates from it is triple prussiate unaltered: that is to say, this gives prussian blue with solutions of red oxide, while the other cannot.

If a lighted candle be applied to the mouth of the retort, the prussic acid burns alone; and the carbonic acid, arising from its combustion, forms with the ammonia crystals of ammoniacal carbonate, which condense in the neck of the retort a few lines beneath the flame. We will now proceed to examine the melted prussiate.

The mass resembles melted muriate of soda, is of an ashen gray, and strongly attracts moisture.

If we taste a bit of it, we find nothing of the sweetness of the triple prussiate, but an alkaline taste, flavoured with the bitterness of the kernels of stone fruit. This flavour announces, that there is simple prussiate of potash in this residuum. A few drops of acid extricate a gas, which does not belong to this prussiate, and which give a suspicion, that it contains carbonate of potash also.

Finally this mass, if set by to dissolve, deposits a black, micaceous, shining powder. On collecting it in a filter, it is found to be a mixture of charcoal, pure iron, and a little sulphuret of iron. The last is an accidental product. Its sulphur proceeds from the decomposition of the sulphate of potash, from which it is not easy to free the triple prussiate. This powder is obedient to the magnet. A weak acid disengages first sulphuretted hidrogen, then aromatic hidrogen, and at length nothing remains but charcoal powder.

Examination of the Solution of the Residuum.

The residuum examined.

If alcohol at 25° be mixed with it, immediately a shining pearly snow is formed, which may be collected on a filter.

Dissolved

Dissolved and crystallized, it affords yellowish crystals, of a sweetish taste, and furnishing prussic acid and white prussiate, when acted upon by muriatic acid. This is the prussiate freed from oxide proposed by Mr. Richter.

The alcoholic solution being distilled almost to dryness, and the residuum covered with alcohol at 30°, one portion is dissolved, and another falls to the bottom. The precipitate is found, on examination, to be carbonate of potash, with a remnant of the triple prussiate. The new solution being distilled affords simple prussiate, which is discoverable by its taste, and by its property of not producing blue with solutions of red oxide. These are the products I have found after subjecting the triple prussiate of potash to fusion.

Consequences.

Recapitulation of the results.

The triple prussiate cannot support a considerable temperature without being simplified in its composition. It frees itself from black oxide, and passes to the state of simple prussiate: but this too is reducible to something more simple, as we shall see below; and it then leaves in its stead potash and the usual results of the prussic acid, ammonia, and carbon. A portion of the latter serves to disoxigenize the black oxide, reducing it to iron, and forming carbonic acid with its oxigen.

During these changes, a part of the triple and simple prussiates escape being acted upon, in proportion no doubt as they become enveloped in the carbonate: but it is to be presumed, that a high and continued heat, in vessels capable of supporting it, would ultimately reduce these prussiates to two binary combinations, which are ammonia and carbonic acid, potash, iron, and some remains of carbon, that the oxigen of the iron and the water were incapable of acidifying.

Simple prussiate of Potash.

Mode of obtaining the simple prussiate of potash.

This is obtained by saturating potash, in Scheele's mode, with prussic acid disengaged from the prussiate of potash or of mercury. But a more expeditious way is keeping alcohol on a concentrated lixivium of animal coal, shaking

it from time to time; and the progress of the solution will be discovered by the alkaline and bitter taste of the alcohol. The lixiviums of charcoal of blood or of leather are seldom exempt from a little hidrosulphuret, because the sulphate that contaminates potash introduces sulphur into them. In this case it enters into the alcoholic solution; but the charcoal contributes to it likewise, for I have prepared lixivia with charcoal of blood and very pure carbonate of potash, and yet found hidrosulphuret in them, though in smaller quantity. It must not be forgotten indeed, that sulphur is found among the products of blood. It even appears, that, like phosphorus, it is capable of fixing in the charcoal, but not combined with the iron it contains; for the aromatic hidrogen, mentioned above, does not afford the least indication of sulphur by its smell.

Animal charcoal affords sulphur.

Characters of the simple prussiate.

The simple prussiate is easily recognized by its bitter alkaline taste, and the aromatic flavour with which it strongly perfumes the mouth. It precipitates solution of copper yellow, and does not afford blue with a solution of the red oxide of iron, but precipitates it of an ochry yellow, as a pure alkali would *. Finally, it affords blue with a solution of common sulphate of iron, because it first constitutes itself a triple prussiate, and afterward produces white or blue prussiate of iron. If the prussiate be black, it is because the alkaline hidrosulphuret introduces into it hidrosulphuretted oxide; but it may be freed from this by a few drops of acid, and the prussiate of iron will appear alone. The simple prussiate does not keep well unless closely stopped. Scheele has shown, that the carbonic acid is sufficient to separate the prussic from the

Blackness from hidrosulphuret removed by an acid.

Must be closely stopped.

* In a memoir on the stone of Sigena, I had mentioned this combination as possible, but it was from mistake. A sulphate of iron, which I had superoxided by nitric acid, retained notwithstanding a portion of black oxide, and this deceived me; and Scheele, whom I contradicted on this point, saw more clearly than I.

In our abridgment of this memoir, Journ. Vol. XII. p. 2. we did not insert the remark here alluded to, as we were persuaded that Scheele was right, and that our author must have been deceived by some circumstance or other. T.

potash, their affinity being so weak. When it is not combined with black oxide, it will not crystallize by concentration, but fixes in a mass, in which however some saline laminæ are distinguishable.

This prussiate is the test liquor proposed by Scheele. Its utility in analysis is very confined; since all solutions, in which the iron is at a maximum of oxidation, and this is the most common case, are not in the least affected by this reagent, as he himself observed. To employ it with utility, part of the oxide of the solution must be brought back to a minimum, which is not easy, or to be done without risk of increasing the difficulties of the process. Of little use as a test.

Its Decomposition.

The aqueous solution of this prussiate gives out part of its acid at a boiling heat, which sufficiently demonstrates, that this combination is neither solid, nor comparable to any of those formed by oxigenized acids. It froths continually, and has something saponaceous. A lighted candle, applied to the orifice of the retort, sets that portion of acid on fire: but its loss is not confined to this, for that portion, which the salt retains more strongly by means of the potash that begins to predominate, likewise experiences a slow but regular destruction from the effect of the heat, which converts it into ammonia and carbonic acid. Examine the product at whatever period of the boiling you please, there will always be found in it carbonate of ammonia mixed with a little prussic acid; and at length, when the water begins to fail, this carbonate condenses in needles in the neck of the retort. Decomposed easily.

If water be supplied, that the boiling may continue, the same products will be found in the water of the receiver; but after four or five successive distillations in the same manner, they cease to be perceptible, though the saline residuum still evidently contains prussic acid.

On treating this residuum with alcohol, part is dissolved, which is found to be prussiate of potash; but the saline matter left undissolved is carbonate of potash. The two following experiments leave no doubt of the destruction of the simple prussiate by a boiling heat. The residuum.

Boiling converts the prussiate into a carbonate.

Prussiate of potash does not render turbid a solution of muriate of lime; but after it has undergone ebullition for some time, it precipitates it copiously in the state of carbonate. The prussiate of potash therefore must have been converted into carbonate of potash.

Experiment.

Two measures of solution of prussiate, one in its natural state, the other altered by long boiling, were employed to precipitate common sulphate of iron. Each afforded blue; but after the brightening, that produced by the former was three times as much as the other.

Decomposed by heat.

If dry simple prussiate be heated to redness, carbonate of ammonia will pass over, contaminated by an oily vapour similar to that of hartshorn. The saline mass being dissolved leaves behind charcoal, and is carbonate of potash mixed with a portion of prussiate not decomposed.

Consequences.

The simple prussiate a weak combination.

All these results unquestionably authorise us to conclude, that the simple prussiate of potash is a feeble combination, as Scheele had already found, the principles of which are easily dislodged, like all that are complex. We see in fact, that part of the acid separates from the potash by the effect of dilatation simply; while another part, subjected longer to the agency of caloric, is destroyed by being changed into ammonia and carbonic acid. Let us proceed to the application.

But not the triple prussiate.

That the triple prussiate of potash is not deranged by repeated ebullition is a fact. The lixiviums employed in manufacturing prussian blue contain, as we shall see below, both the triple prussiate and the simple prussiate. There is not found in them, however, any ammoniacal salt. It might be presumed, indeed, that the great excess of carbonate of potash they contain would be incompatible with such a salt; yet they evolve ammonia, as long as they continue in ebullition. Whence then can this ammonia proceed, if not from the decomposition of the simple prussiate? We may infer, therefore, that boiling the lixivia, or concentrating them by evaporation, is liable to injure them by the destruction of that very prussiate, which cannot be too sedulously preserved; and as the carbonate of potash

Boiling the lixivia in manufactories of prussian blue injurious.

potash is likewise one of the principles resulting from this destruction, it does not cease to add to what is found there already.

Curaudeau was aware of the injury occasioned by boiling the lixiviums, and happily prevented its effects, by adding to them a little sulphate of iron, agreeably to the principle of Scheele, who made known, that the simple prussiate was converted into a triple prussiate, whenever it could acquire a portion of black oxide, and thus defended itself from decomposition. As to the products of the destruction of the prussiate by fusion, or by ebullition, undoubtedly there is nothing extraordinary in them, since it is sufficient for us to be acquainted with the nature of the prussic acid to foresee them; but it is not the same with respect to the carbonic acid, which presents itself during one of these destructions. Whence, for instance, comes the oxigen, which, during the ebullition of the aqueous prussiate, acidifies the carbon of the prussic acid? Either this oxigen must be one of the principles of the prussic acid that is destroyed, or we must suppose, that a decomposition of water has taken place. I do not think we are yet sufficiently advanced, to choose between these two opinions; but till we have a clearer insight into the subject, I cannot help saying, that, if we reflect on the circumstances accompanying the production of the prussic acid, we shall be more inclined to adopt the opinion of Berthollet, than any other hypothesis. His words are: "it appears to me difficult to conceive the existence of oxigen in a substance, which contains elements so strongly disposed to form particular combinations with it, as hidrogen and carbon, and yet is capable of enduring a pretty high temperature without being decomposed." In fact, to admit that this acid is an oxigenized compound, we must suppose, that such an acid is capable of disputing oxigen with the carbon by which it is surrounded on all sides; and not only place it at the head of the acids, but even of those oxides which are known to be most difficult of reduction.

unless a little sulphate of iron be added,

to form the triple prussiate.

Whence the carbonic acid formed?

Most probably no oxigen in prussic acid.

(To be continued.)

II. *A Table*

II. *A Table of the Growth of Trees in the Botanic Garden at Calcutta.* By Dr. Wm. Roxburgh.*

THE average circumfererence of several of each sort of trees mentioned in the list, in inches and quarters, was taken 4½ feet above the ground, at the end of October, when the year's growth may be reckoned over. The difference between the numbers in the adjoining columns gives one year's increase in the circumference of each sort.

Names of the Trees.	Synonymas.	Years Age of the Trees measured in 1803 and 1804.																		
		2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Tectona grandis	Teak, E. Sagoon, H.	—	7	10½	—	—	—	—	18½	21½	—	—	—	—	—	42½	44½			
+Swietenia Mahogani	Mahogany, E.	—	—	—	—	—	25½	30½												
Swietenia chickrassa	Chickrassy, H.	—	—	—	—	—	—	—	25	28										
Swietenia febrifuga	Soymida, T. Rohcena, H.	—	—	—	—	—	—	—	21½	24½										
Terminalia alata-tomentosa	Asseen	—	—	—	—	—	—	—	—	—	—	—	—	—	37	38				
Terminalia bialata		—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	51	53½	
Terminalia catappa	Buddam	—	—	—	—	—	—	—	—	30	32½									
Terminalia procera		—	—	—	—	—	—	—	23	25										
Terminalia belerica	Beheyrah, H.	—	—	—	—	—	—	—	—	20	23½									
‡Dalbergia sissoo	Sissoo, H.	5	11½	14½	17½	—	—	—	—	—	—	—	—	—	—	—	—	—	70	75
Dalbergia ougienensis		—	—	—	—	—	12½	14½												
§ Dalbergia emarginata		—	—	—	—	—	—	—	—	—	—	—	—	25½	30					
Pterocarpus marsupium	Yeangashaw, T.	—	—	—	—	—	—	—	—	—	—	23	26							
Pterocarpus Dalbergioides	Andaman, red-wood	—	—	—	—	—	—	—	—	27	29½									
Cedrela toona	Toon, or Tooni, H.	—	—	—	—	—	—	—	—	—	—	32½	35	—	42					
Mimosa seereesa	Seerees, H.	—	—	—	—	—	—	29	31½											
Mimosa Xylocarpa	Conda-tangheroo, T.	—	—	—	—	—	17	21												
Mimosa elata		—	—	—	—	—	—	—	—	28	30½									
Mimosa stipulacea	Amlooki	—	—	—	—	—	—	40	48½											
Mimosa lucida		—	—	—	—	—	—	—	—	—	—	49	56½	—	—					
Pterospermum paniculatum	Mows-jal, H.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	21½	24½			

Table continued.

Names of the Trees.	Synonymas.	Years Age of the Trees measured in 1803 and 1804.																		
		2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Aleuritis triloba		—	—	—	—	—	28½	31												
Nauclea stipulacea		—	—	—	11	13½														
Nauclea nicotianifolia		8	15¼																	
Uvaria odorata		—	—	—	14	19														
Casuarina muricata, in garden mould		—	—	—	20	22														
The same in very sandy soil		—	—	—	26½															
Cassia Sumatrana		17½	23½																	
Butea frondosa	Palas, H.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	40	43
Lagerstromia Reginæ	Jaral, or Jarool, H.	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	48½	53
Gmelinia arborea	Gumar, or Gumhar, H.	—	—	18½																
Melia azedarach		—	—	—	—	37½														
Bignonia suberosa		—	—	—	—	—	—	—	—	—	36	39¼								
Sterculia villosa		—	—	—	—	—	—	25	27½											
Andersonia altissima	Siri-Maun, T.	—	—	—	12½	15½														
Myristica aromatica	Nutmeg	—	—	—	—	3½	4½	Two feet and a half above the ground.												

* From the Transactions of the Society of Arts, for 1806.

† Two plants (and the first) were sent by the Honourable Court of Directors, to the Botanic Garden at Calcutta, in 1795, and by the end of 1804, above 500 plants had been reared from these two. It succeeds so perfectly well in the East Indies, that the tree will, without doubt, in a few years, prove a valuable acquisition to that country.

‡ An immense tree of very quick growth. It supplies the ship-builders at Calcutta, with all the knee, and other crooked timber. It is strong, but not very durable.

§ This promises to be more durable than Sissoo. It is a native of the Andaman Islands. As yet, the trees on the continent of India are small, and but few in number.

Note. In the column of Synonymas, H. means Hindoostanee, T. Telinga, and E. English.

The Specimens noted in the following Letter are placed in the Society's Repository.

DEAR SIR,

Ejoo, the fibres of the leaves of

I think it more than probable that the Society of Arts, &c. do not possess any specimen of the vegetable fibres, commonly called Ejoo in the East Indies. I have, therefore, the pleasure of sending you a parcel of that substance, consisting of six small rolls, the produce of a small tree for one year. The tree produces, on an average, six leaves every year, and each leaf yields from about four to twenty ounces. It is No. 4. of my first paper on the comparative strength of various vegetable fibres, published in the xxii. vol. of the Society's Transactions. A description of the tree has lately been published by Labillardiere, under the name of *Aronga Saccharifera*. It is the *Anou* of Marsden in his History of Sumatra, page 77: in Rumphius's Herbarium Amboynense, vol. i. page 57, and table 13. a very full account of this valuable palm will be found. By Loureiro, in his Flora Cochinchinensis, p. 759, it is called Borasus Gomutus. The cultivation of this beautiful, stately, and very useful palm may, I think, with the prospect of great advantage, be encouraged in the West Indies. For, besides the above-mentioned fibres, which are in high estimation for thick cordage and cables in India, this palm furnishes sugar, and abounds, as before mentioned, probably more than any other, in wine, which, in its recent state, is a pleasant and wholesome beverage, and is also converted by the Malays into ardent spirits; and when the tree arrives at maturity, the pith of it is one of the varieties of sago meal used by these people in their diet.

the Arenga Saccharifera.

Made into ropes.

Affords much wine and sugar.

A substitute for cork.

I have the pleasure also of sending you a specimen of a most curious, light, vegetable, substance, the spreading stems of *Aeshynomene Aspera*, a water plant, called by the Hindoos and Bengalese *Solah*, and *Fool-Solah*. It is employed by them for a variety of purposes, such as floats for fishing nets, artificial flowers, &c. Might it not be advantageously employed instead of cork, in making jackets to swim with, and in life-boats, &c.? At all events, the bare circumstance of making known the existence of such a plant,

plant, and the place in which it flourishes, will, I am persuaded, be acceptable information to the Society.

I am, Dear Sir,

Your most obedient humble Servant,

W. ROXBURGH.

III.

Extract from a Dissertation on Coffee, its History, Properties, and the Mode of obtaining from it the most pleasant, wholesome, and economical Beverage: by ANTONY ALEXIS CADET DE VAUX, *Member of various Academies: with its Analysis, by* CHARLES LEWIS CADET, *Apothecary in ordinary to his Majesty the Emperor, Professor of Chemistry, &c.**

PASSING over the historical part, which is sufficiently known, we shall confine ourselves to the chemical and economical.

Raw Coffee treated with Water.

When boiling water is poured on coffee as we find it in the shops, it acquires a yellowish green colour.† If the action of heat be continued, the decoction grows brown, and a light scum rises, which remains insoluble. The decoction passes clear through the filter, but becomes turbid on cooling. A little caustic potash poured into this decoction gives it a deeper brown, and ammonia produces a similar effect. Lime water forms in it a copious flocculent precipitate. Sulphate of iron converts it into a black ink. Solution of gelatine is not rendered turbid by it. Oxigen-

Decoction of raw coffee treated with reagents.

* Journal de Physique, Vol. LXIII. p. 216, Sept. 1806.

† When coffee is fresh gathered, its decoction is of a fine emerald green. A lake might be made of it, and Mr. Dupont de Nemours informs me, that in the West Indies it is used for washing and colouring maps.

Lake from coffee.

ized

ised muriatic acid deprives the decoction of its colour but in part; and, if an alkali be added to this mixture, it becomes red.

Distillation.

Distilled yielded a volatile oil. I distilled eight pounds of water with a pound of raw coffee, and obtained an aromatic water, on the surface of which were a few drops of a concrete oil, similar to that of the *myrica cerifera*, or candleberry myrtle. The decoction remaining in the still was viscous. This I diluted with a little water, and poured into it alcohol. A copious precipitate was thrown down, which, collected on a filter, was soluble in water, and had all the characters of mucilage. The coffee from which the water had been distilled, being dried in a stove, and digested in alcohol, afforded a tincture, which gave a precipitate on adding water.

The decoction gummy.

Resin left in the coffee.

The decoction turned litmus green, The aqueous decoction of raw coffee does not redden vegetable blues. With litmus it even produces a green. All the chemists who had analysed coffee before me have said, that the decoction held in suspension a free acid, which reddened blue vegetable colours. Geoffroy even went so far as to assert, that water distilled from coffee by the heat of a water bath was rendered very sour. I have tried five different sorts of coffee, and repeated the experiment more than twenty times, but the decoction never appeared sour to me.

and contained no acid.

Decomposed alum. It decomposes sulphate of alumine, and precipitates its earth, which it colours slightly.

Raw Coffee treated with Alcohol.

Alcohol extracts its resin, Alcohol becomes slightly tinged by standing on dry coffee, even without heat, and holds in solution a considerable quantity of extracto-resinous matter. If water be added to this tincture, it turns milky, and the resin falls down of a dirty white colour. With a solution of sulphate of iron the precipitate is green; with muriatic acid it is fawn-coloured. The coffee exhausted by alcohol, and afterward treated with water, still furnishes extractive matter and mucilage.

leaving extract and mucilage.

Immediate principles of From these preliminary experiments we may conclude, that raw coffee contains, 1. an aromatic principle soluble in water;

water: 2. a very small quantity of essential oil: 3. a resin in tolerable abundance: 4. a gum in greater quantity: 5. gallic acid, but no tannin: 6. extractive matter: 7. a little albumen. raw coffee.

Observations.

If the decoction filtered while hot become turbid on cooling, it is because it holds in solution by means of the heat a little resin. Alkalis render it brown; their usual effect on vegetable decoctions. Lime water precipitates it, because on the one hand gallate of lime is formed; and on the other the gummy extractive matter unites with the earth, and carries it down. The same may be said of the sulphate of alumine. Spirit of wine separates the mucilage, because gums are not soluble in alcohol; and water precipitates the alcoholic tincture, because resins are insoluble in water. This precipitate by water is white in consequence of its extreme division: by sulphate of iron green, because it is mixed with gallate of iron: by oxigenized muriatic acid fawn-coloured, because the oxigen, attacking the resin, sets bare a little carbon. The insoluble scum formed on the surface of the decoction is a little vegetable albumen coagulated by boiling water. To obtain this, it is necessary, that the water should stand some time on the coffee cold, before it is heated. General remarks on these.

Proportions by Approximation.

Though it is not of much use to inquire into the proportions of the immediate principles of coffee, since these proportions must vary as the berry is more or less ripe, and according to the place from which it comes, and the time it has been kept, I have deemed it not superfluous, to estimate them as nearly as may be. After several comparative experiments I have found, that eight ounces of coffee afford nearly Proportions of its principles.

	oz.	dr.	gr.
Of mucilage	1	0	0
Resin	0	1	0
Colouring extractive matter	0	1	0
Gallic acid	0	3	36
Parenchyma	5	3	36
Vegetable albumen	0	0	10
	7	1	10

It

Coffee germinates in boiling water; but not in alcohol.

It has long been known, that coffee germinates in boiling water (see Bomare's Dict. art. *Café*,) and I have verified this fact. But it does not germinate in boiling alcohol; either because the temperature is not sufficiently high, or because the water is necessary to its germination, or because alcohol destroys its vegetative action.

Martinico and Bourbon coffee alike.

I have compared the decoctions and tinctures of three different sorts of coffee; those from Mocha, Martinico, and the Isle of Bourbon. The last two appeared to me to furnish the same principles in the same proportions, but that of

Mocha different.

Mocha differs essentially from the others. Its decoction was much less saturated, its alcoholic tincture was higher coloured; it contained less gum and less gallic acid, but more resin and more aroma.

Torrefaction.

To know what changes are produced in coffee by roasting, I examined the phenomena that take place during its torrefaction in the open air.

Effect of roasting.

At first the coffee, being penetrated by the caloric, increases in bulk: it crackles, and becomes fawn-coloured: the arillus or pellicle that envelops the seed, separates, and as it is very thin and light the least breath blows it away. The coffee then diffuses a very agreeable aromatic smell. This vapour grows more intense; the seed smokes, and turns brown: presently the smell changes, and becomes slightly empyreumatic; the coffee sweats, and becomes oily on its surface*; it ceases to smoke, and if the action of the fire be continued it is carbonized.

Difficult to roast it to a proper point.

The interval that separates the instant that the coffee becomes coloured from that of its carbonization is sufficiently long, to render it difficult to determine the point at which we should stop, in order that the berry may retain its most agreeable properties; but in order to approximate this point,

Fat oil in coffee.

* Mr. Parmentier wrapped up some roasted and sweating coffee in blotting paper. This paper, imbibing the oil, remained greasy and transparent for more than a year, which indicates the existence of a fat oil in the berry. I could not separate any such oil, however, either by pressure, boiling in water, or the action of caustic alkalis,

which

s of so much importance to be known, I divide the Three stages of this process.
of roasting into three distinct periods: 1, that in
the berry loses its natural colour, and assumes that
d raspings, or dried almonds: 2, that in which it
s the brown red of a dry chesnut: 3, that in which,
almost black, it is still not charred.
k six ounces of Martinico coffee, divided them into Experiment.
arts, and roasted them separately in these three de-

two ounces slightly roasted, and of the colour of Lost of weight $\frac{1}{8}$.
lmonds, lost on the fire two drachms. These I shall
. 1.
two ounces roasted to a chesnut colour lost three $\frac{3}{16}$,
s. These I call No. 2.
two ounces roasted to blackness lost three drachms or near $\frac{1}{4}$.
ight grains. These I shall distinguish as No. 3.
1. passed through the mill with difficulty. Infused Slightly roasted.
ie infusion contained tannin, and precipitated solu-
f gelatine; it was very aromatic,* and had the fla-
f almonds; there was not the least bitterness, but a
atly decided harshness. Infused hot its aromatic fla-
as the same; and its taste reminded me of that of
nond cake called *nougat*. It was not at all bitter,
harshness was less perceptible.
2. was more easy to grind. To cold water it gave Higher roasted.
s tannin; its aromatic flavour was weaker, and it had

e object of retaining the aroma, which is dissipated by a Two methods used to retain the aroma:
heat, has given rise to two processes, which are not alto-
neffectual. The first, adopted in India and by some per- butter in the roasting;
France, consists in putting into the cylindrical roaster a lit-
butter, when the coffee begins to be coloured. No more
used than will slightly varnish the surface of the berries.—
tter retains a part of the essential oil, that would have eva-
. It is not a bad method, but sometimes it imparts to the
peculiar flavour, which every body does not like.
econd consists in spreading the roasted coffee, while yet hot powdering with sugar after it.
eating, on writing paper, and powdering it lightly with su-
The sugar absorbs the oil of the coffee, and retains its aroma;
oes not appear to me to increase the pleasantness of the cof-
d renders us uncertain how much sugar to put into a cup.

more

more of the taste of burnt sugar, but neither bitterness nor harshness. Infused in hot water it gave out neither more taste, nor more aroma.

Very highly roasted.

No. 3. was reduced to powder very easily. To cold water it imparted scarcely any aroma: its taste was empyreumatic, and slightly bitter: and the precipitate it afforded with solution of gelatine was hardly perceptible. The infusion in hot water was more bitter, more empyreumatic, and had a more distinct aroma.

Roasting increased the gum and acid, diminished the tannin.

All these infusions contained mucilage and gallic acid, but in an inverse ratio to the tannin; for the proportions of gum and acid increased with the degree of torrefaction, while the tannin diminished.

Gallic acid a modification of tannin.

Mr. Bouillon Lagrange, in an excellent paper on galls,* had already considered the gallic acid as a modification of tannin; and these experiments tend to confirm his opinion.

Examination of the Roasted Coffee.

As the immediate principles of coffee are not equally soluble or volatile, it was necessary to make a comparative examination of the hot and cold infusions of the three sorts of coffee, as well as of their decoctions.

Infusion in Cold Water.

Roasted coffee macerated in water,

I poured eight ounces of distilled water on one ounce of roasted and ground coffee, and after they had stood together two hours, I filtered the liquor. The infusion was of a very clear brown, did not redden blue paper, was blackened by sulphate of iron, and slightly precipitated solution of gelatine. Alcohol separated from it a little mucilage, and gave the infusion the smell of juniper. Mocha, Bourbon, and Martinico coffee exhibited the same characters.

Hot Infusion.

Infused,

I infused an ounce of roasted and ground coffee for a quarter of an hour in eight ounces of water at 70° (190° F.) This infusion did not redden litmus, or precipitate solution of gelatine, but formed ink with sulphate of iron. Alco-

* See Journal, p. 58 of the present Volume. T.

hol

hol separated more gum from it than from the cold infusion. The three sorts of coffee comported themselves the same in these experiments.

Decoction.

I boiled two ounces of powdered coffee in one pound of water, and continued the boiling for two hours. The smell of the decoction was infinitely less agreeable and aromatic than that of the infusion. It did not change the colour of blue paper, or precipitate the solution of gelatine, but was blackened by sulphate of iron. Alcohol separated from it much more mucilage than was found in the infusions, in proportion to the quantity of coffee. The three sorts of coffee afforded the same results. Boiled.

If a filtered and limpid decoction of coffee be boiled a long time exposed to the air, it grows turbid, and deposits a black powder, which has sometimes been mistaken for resin, but is only extractive matter highly oxigenized. Physicians and apothecaries have not yet sufficiently examined the action of the air on vegetable decoctions; but they might derive from it some information respecting the more or less active properties of certain medicines. Effect of long boiling.

Extract of Coffee.

The decoction of coffee, when filtered and evaporated to the consistence of an extract, has no longer the aromatic odour of the infusion. Its taste is bitter. Heated with alcohol, this extract colours it with its extractive matter, but the tincture affords no precipitate on the addition of water. Hence we may conclude, that the decoction of coffee, after it has been filtered or stood to settle, contains no resin. Extract.

Spirituous Tincture of roasted Coffee.

Roasted coffee digested in alcohol affords a high coloured tincture, from which water precipitates a larger quantity of resin, than from the tincture of dry or raw coffee.—From the latter the resin is white: from the tincture of roasted coffee it is fawn coloured. Tincture. More resin in roasted than raw coffee.

Observations

Observations.

Effects of roasting. It follows from these experiments, that roasting develops the odorant and resinous principles of coffee, and forms in it tannin, which is soluble only in cold water. This is a very singular phenomenon. Gallic acid manifests itself in coffee, at every temperature of the water employed as a menstruum. The gum and colouring extractive matter are more abundant in the decoction than in the infusions; but the aromatic principle is more perceptible and more agreeable in the latter.

Distilled Water of roasted Coffee.

Water distilled from roasted coffee. I distilled several quarts of water from roasted coffee. The water was impregnated with the aroma of the coffee, and carried over with it some atoms of concrete essential oil, like that obtained from the distillation of raw coffee. Reagents did not demonstrate the presence of any substance in solution in this water.

Infusions and Decoctions compared.

Treated first with cold water To find the different solubility of the principles of coffee, it remained for me to subject the same powder of roasted coffee to the successive action of infusion and decoction. For this purpose I placed in a filter two ounces of coffee, and passed cold water through it, till the reagents ceased to indicate the presence of the matters in solution. Sixty-eight ounces of cold water were necessary to divest the coffee of all the matter thus soluble. I divided this water into seventeen portions of four ounces each, as they passed through the filter. All these contained gallic acid in proportion to the order in which they passed through: the first four took up gum; but only the first indicated the presence of tannin, by precipitating a solution of glue.

then with hot, The coffee having been taken out of the filtre, and dried on a stove, I poured on it eight ounces of water at 75° (201° F.) The smell of this secondary infusion was pleasant, but weaker than that of coffee prepared for the table. Examined by reagents it furnished a little mucilage, and a great deal of gallic acid; but I found in it neither tannin nor resin.

I took

I took this same coffee, already washed with cold water, and infused in hot, and boiled it in six ounces of water, till they were reduced to four. This decoction contained a great deal of gum and gallic acid, but little aroma, and afforded no trace of tannin or resin with reagents. and lastly boiled.

Observations.

These experiments prove, that cold water divests roasted coffee of the little tannin it contains, of part of its extractive matter, and of great part of its aroma; but that it takes up only a small portion of its gallic acid, and of its gum. We perceive, that the hot infusion is more loaded with both of the latter principles; but that its aroma is weaker. Lastly we find, that long boiling dissipates in a great degree its odour, but is highly loaded with gum and gallic acid. If it be found to contain resin, this is only suspended in it, disturbs the transparency of the liquor, and is deposited by standing. Effects of cold water, of hot, of boiling.

Ashes of Coffee.

Though it is of little importance to know what coffee reduced to ashes contains, I incinerated about half a pound. The ashes were pretty light. Lixiviated with distilled water, their analysis afforded nothing but a little lime, and a very little potash. I acidulated the lixivium with a small quantity of nitric acid, and the filtered solution precipitated prussiate of potash of a fine blue. Oxalic acid gave with it a copious precipitate. It was not altered by barytes. Nitrate of silver turned it white. Coffee ashes then are composed of carbon, iron, lime, and muriate of potash. I did not think it necessary to ascertain their proportions. Incinerated, and the ashes examined, contain carbon, iron, lime, and muriate of potash.

I had intended here to have concluded my analysis, when Mr. Parmentier read at the Society of Pharmacy a very copious memoir on coffee, written by Mr. Payssé, an apothecary, who has already published several very interesting works. It is said in this memoir, 1st. that the precipitate formed by the mixture of the decoction of coffee * Coffee analysed by Mr. Payssé.

* This is a mistake. It was the precipitate formed by the acid of coffee, as Mr. Payssé calls it, obtained in the way in which Mr. Chenevix found what he considers as a distinct principle, by precipitating the decoction with muriate of tin, and separating the tin by sulphuretted hidrogen. T.

in their physical properties; yet all mucilage, all saccharine matter, is the same chemically considered*. Proust has demonstrated, that tannin obtained from different vegetables displayed some differences: it is possible, therefore, that gallic acid obtained from coffee may not be absolutely the same as that from galls, but it is not a distinct acid.

Recapitulation.

Principles of coffee.

It appears to be demonstrated from the analysis above given, that the coffee berry contains abundance of mucilage, a great deal of gallic acid, a resin, a concrete essential oil, albumen, and a volatile aromatic principle. To these principles are added those found in many vegetables; namely, lime, potash, iron †, carbon, &c. Torrefaction develops the soluble principles; but it must be moderate, if we would retain the aroma, and not decompose the acid, the gum, and the resin.

Effects of roasting.

Tannin produced by it. Treated with water.

The roasting adds a new principle, which is tannin, though in very small quantity. The cold infusion is very aromatic; but it contains little mucilage or gallic acid. The hot infusion retains some of the aroma; and the principles dissolved in it are in such proportions, as to be agreeable to the taste. The decoction has little aroma, and is much loaded with gum and gallic acid, the resin too may even be suspended in it, and it is less pleasant to the taste than the infusion.

Different sorts of coffee.

The coffees from the island of Bourbon and Martinico

* The fecula of potatoes does not resemble that of wheat, and this again differs from the fecula of cassada, sago, salep, arum, maize, &c. Yet chemists would say of all these, that it is an amylaceous substance, and find in them the same leading characters.

Iron in coffee with gallic acid, yet not indicated by colour.

† The presence of iron in vegetables is very common; but that of iron in a vegetable containing a great deal of gallic acid, without this acid being combined with it, and imparting a blue or black colour to the vegetable, is a very remarkable phenomenon. It appeared to me deserving of inquiry, and I made a comparative analysis of the ashes of galls, in which also I found a sensible quantity of iron. [It may be observed, however, that galls have very often an evident blue tinge; so much so, that it is commonly considered as an evidence of superior quality. T.]

do not perceptibly differ from each other; but that from Mocha, as was observed above, is more aromatic, less gummy, and more resinous. It is probable, that the resin of coffee, as that of most astringent vegetables, has peculiar medicinal properties. As it is obtainable neither by infusion nor decoction in water, the habitual use of coffee can afford us no insight into its action on the animal economy. It is for physicians to make such experiments on the subject, as they may deem useful.

Perhaps the resin medicinal.

If I might be allowed from this analysis to draw precepts applicable to the domestic use of coffee, I would say, that it is possible to make excellent coffee from every kind of the berry found in the shops, provided it be not damaged. Amateurs look to three points of perfection in coffee: they would have in it an agreeable aroma, a slightly rough taste, and a certain density, which is called *body**. All these objects, I believe, may be obtained, by proceeding as follows.

Domestic use of coffee.

1. Choose a coffee, that, when dry, has no taste of mouldiness, or is not damaged by salt water.

How it is to be made in the greatest perfection.

2. Divide the quantity to be roasted into two equal parts.

3. Roast one portion only till it is of the colour of dry almonds, or bread raspings, and has lost one eighth of its weight.

4. Roast the other till it is of a brown chesnut colour, and has lost nearly one fifth of its weight.

5. Mix both these together, and then grind them.

6. Let the coffee be both roasted and infused the day on which it is to be drunk.

7. Pour four cups of cold water on four measures, or two ounces of coffee, and when the water has run off, set it by.

8. On the same coffee pour three cups of boiling water, and mix the water that runs off with the preceding. You should thus have six cups of coffee.

* Some of the eastern nations value this density so highly, that they reduce their coffee to a very fine powder, leave the grounds in the infusion, and drink it as thick as a kind of thin pap.

9. The

9. The moment you are going to drink the coffee, heat it over a brisk fire, but do not let it boil.

10. The infusions should be made in a china, earthenware, or silver pot.

Such is the process pointed out by theory, and I can recommend it from experience.

Payssé's analysis of raw coffee.

To give the whole of Mr. Payssé's memoir, alluded to above, would occupy too mueh room; but we apprehend it will be acceptable to the reader, to have subjoined the conclusions which that gentleman draws from his chemical investigation of raw coffee; particularly as he differs, in some respects, both from Mr. Cadet, and from Mr. Chenevix.

It contains a peculiar acid,

1. It results from all these experiments*, that coffee contains a peculiar acid sufficiently characterised: that it is in some respects free, since the powder of the berry speedily reddens blue vegetable tinctures: and that cold water, or even alcohol, can separate it in a state more or less pure.

which decomposes most metallic solutions.

2. That the acid decoction of coffee easily decomposes most of the metallic solutions, as those of tin, lead, iron, &c.

Precipitates of these by the decoction more copious, because less pure.

3. That the precipitates obtained by a mixture of this decoction with the metallic solutions are more copious than those formed by the pure acid, because the decoction contains extractive matter, colouring matter, albumen, &c. beside the acid. For the colouring matter is partly precipitated by the affinity it has for the compound, formed of the coffic acid with the metallic base; and on the other hand the albumen, being separated from the acid which promoted its solution in the liquid, falls down and increases the bulk of the precipitate. To be convinced of the truth of this, nothing more is necessary, than to boil a coffat of tin, lead, or alumine, in a coloured vegetable decoction, to obtain the result in question.

Methods of obtaining the acid.

4. That the acid of coffee may be obtained sufficiently pure by mixing a decoction of coffee in water, or a tincture of it in alcohol, with the muriate of tin or of lead, and afterward decomposing this combination by sulphuretted

* *Annales de Chimie*, Vol. LIX. p. 196. August, 1806.

hidrogen,

hidrogen, as Mr. Chenevix did; or by decomposing coffat of lead by the sulphuric acid.

5. That this new acid is not crystallizable in the state in which I obtained it; but is completely soluble both in water and in alcohol. *It is soluble in water and in alcohol.*

6. That it is capable of decomposing the prussiate of iron contained in the prussiate of potash, forming with this metal a green precipitate: and in this respect it may be of great service to chemists for obtaining prussiate of potash perfectly pure, which hitherto they have been unable to deprive of a certain portion of iron, it retaining this with so much obstinacy. *Decomposes prussiate of iron, and forms a green precipitate.*

7. That the colour it communicates to the oxigenized and green sulphate of iron appears altogether new *. *Its effects on sulphate of iron.*

8. That the attraction of the compounds it forms with tin, lead, antimony, and alumine, for the colouring part of vegetable decoctions or infusions, may render it of use in the art of dyeing. *May be of use as a mordant.*

9. That the different kinds of coffee contain it in nearly the same proportion; and that it exists without alteration, though in smaller quantity, in the infusions and decoctions of coffee roasted in different degrees, as well as in the products of its distillation. *In all kinds of coffee, raw or roasted.*

10. That the comparison I made of the properties of this acid with those of the gallic acid and tannin did not show me any identity of nature between these three very different substances. *Differs both from gallic acid and tannin.*

11. That the peculiar principle obtained by Mr. Chenevix was, no doubt, the acid substance in question; though it was not examined with sufficient strictness by that learned chemist. *It was the peculiar principle obtained by Chenevix.*

12. That, having examined the infusions and decoctions of different sorts of roasted coffee, they did not afford me any proof of the existence of tannin, by mixing them with gelatine, as Mr. Chenevix asserts. *No tannin in roasted coffee.*

* Coffic acid, dissolved in six times its weight of water, added to a solution of oxigenized sulphate of iron, immediately gave it a fine green colour; and after it had stood six hours, a precipitate of the same colour fell down. To a solution of green sulphate it gave at first a very light green tinge, but this grew deeper, after it had been some time exposed to the air.

13. That

Coffic acid unites with various bases,

13. That the acid of coffee is capable of uniting with a great many bases, and forming peculiar salts, decomposable with more or less facility by fire, and the powerful acids; and that its affinities appear to follow a law altogether different from that of most of the known acids, since its union with alkalis seems to be the weakest.

but most feebly with alkalies.

Decomposed and reduced to malic acid.

14. That it is decomposed by hot sulphuric acid, and the nitric, muriatic, and oxigenized muriatic acid; and reduced by the latter, as well as by the nitric, to malic acid.

Contains much carbon with little hidrogen.

15. That, from the products obtained by its analysis by fire, it appears to be composed of a great deal of carbon, with less hidrogen and oxigen.

Component parts of the watery extract.

16. That 100 parts of aqueous extract of coffee, the product of about 750 parts of the berries, afforded me of coffic acid 55, extractive matter 25, vegetable albumen 5, and resinous matter 9; the loss being 6.

17. That, to adopt the language of modern chemistry, this acid ought to be called the *coffic*, from the name of the substance from which it is taken.

Ashes.

18. That the incinerated residuum of coffee is composed of muriate of potash, lime, and a portion of iron, the quantity of which was too small to be ascertained.

Remote principles of coffee.

19. Finally that coffee, from all that has been said, is a substance containing carbon in much larger proportions than hidrogen, oxigen, or azote; the existence of all these having been evidently demonstrated by the formation of oil, pyromucous acid, carbonic acid, and ammonia united with this acid, in the destructive distillation of coffee.

IV.

Account of the Existence of Platina in the Silver Mines of Guadalcanal, in the Province of Estremadura. By M. VAUQUELIN *.

Platina found only in South America.

HITHERTO platina had been found only among the gold mines in South America, at Santa Fe, and in the bailiwick of Choco. There was a report a few years ago,

* An. de Chim. Vol. LX. p. 317, Dec. 1806.

that

that platina had been discovered in Siberia; but this has no more been confirmed, than that spread fifteen years ago, of its existence in a ferruginous sand at St. Domingo.

Old mines in Estremadura, lately reopened, furnish platina,

Having been lately employed to analyse the ores of the celebrated mines of Guadalcanal, in Estremadura; which, after having been shut up for a long time, have lately been opened again at a fresh place; I discovered in one variety of these ores the presence of a large quantity of platina.

from a variety of gray copper ore.

This ore is of a gray colour, and bears considerable resemblance to that known in France by the name of gray silver ore, the *fahlerz* of the Germans, [properly gray copper ore.] It contains copper, lead, antimony, iron, sulphur, silver, and sometimes arsenic. Its gangue most commonly consists of carbonate of lime, to which are added sulphate of barytes and quartz. In the month of October last, I communicated this discovery to my learned colleague, Mr. Fourcroy, whose knowledge and friendship have been continually serviceable to me for these twenty years. This fact, which appeared to him highly important, he persuaded me to verify, by experiments so numerous and varied, that they should be open to no dispute. I followed his advice; and the following are the results of my researches, which have left no doubt in my own mind, though hitherto I have been able to operate on no considerable quantities of ore.

It is in various proportions, from 10 per cent. to almost nothing.

The platina appears to exist in various proportions in the silver ores of Guadalcanal. Some specimens afforded me as much as twenty marks to the hundred pounds, or ten per cent; and some exhibited merely traces of it, that were scarcely perceptible; which indicates, that it does not form an essential, or properly constituent part of the ore, and that it is simply mixed in irregular quantities in various parts of the vein.

The silver from two to seven per cent.

The silver appears to be in the same case. In fact this varies greatly in its proportions, as I have found in the gray ore of Guadalcanal from four marks to fourteen, or from two to seven per cent of the whole weight.

Mode in which it was extracted.

The process I employed to extract the platina from these ores, after several comparative trials, consists in the following operations. 1. After having reduced the ore to a fine

fine powder, I roasted it with a gentle heat, stirring it constantly, to avoid the fumes. 2. I then fused it with an equal quantity of common potash, and thus obtained a metallic button, consisting of platina, silver, lead, copper, and sometimes a little antimony. The iron and part of the lead remained in the scoriæ. 3. I then separated the copper, antimony, and remainder of the lead, by cupellation; which left me only the silver and platina. 4. I parted the platina from the silver by means of aqua fortis, or the nitric acid of the shops, which dissolved the silver, and left the platina behind. This I washed, and heated again, to give it the metallic lustre. 5. If the lead naturally found in the first metallic button were not sufficient, to carry off all the copper in the process of cupellation, I subjected the metal to this operation a second time with a fresh portion of lead. 6. On the contrary, if the quantity of silver were too small to allow the aqua fortis to act on the alloy, I added a fresh portion of this metal, as in parting it from gold. 7. I ought to caution the reader, that the aqua fortis, if it be not sufficiently diluted, will dissolve a portion of platina at the same time with the silver; which is easily perceived by the brown colour the solution assumes.

Parting necessary to obtain the platina, and even the silver.

If platina be found in the gray ore of Guadalcanal in a proportion that will allow it to be extracted with advantage, of which, according to my first researches, there can scarce be any doubt, it will require to be parted by means of aqua fortis, in the same manner as is practised with respect to the gold extracted from silver ores: and even if there be no advantage to be derived from the platina extracted by this process, it will be necessary to employ it to obtain the silver; for by any other mode these two metals will be found united together from the similarity of their properties.

It is in the metallic state.

Platina appears to exist in the metallic state in these ores, for the simple acids do not dissolve the smallest quantity of it, and it is constantly found among the sulphur and silex, when the latter constitutes part of the gangue. It was indeed by examining these residuums of the ores, and treating them in succession with nitric and muriatic acid, that I first perceived the platina.

What

What is remarkable on the present occasion is, that neither of the four metals recently discovered, which accompany platina in the ore from Peru, is found in that of Spain. This is a consideration of much importance, since it will greatly influence the means of extracting this metal, and since it gives hopes of obtaining it in a state of purity, which cannot be attained with the platina of Peru, but by means of difficult processes and great expense.

Neither of the four new metals found with it.

Will afford pure platina.

If these hopes be realized, as every thing tends to persuade us, we shall have in Europe, and at hand, a precious metal, which will soon become of great utility for the purposes of natural philosophy, chemistry, the arts, and even domestic economy, in fabricating a variety of instruments, vessels, and utensils of every kind; since, with all the advantages that gold enjoys, it unites several properties, that render it greatly superior to gold*.

V.

Carbonization of Turf, or Process by which all possible Advantage may be derived from Products hitherto neglected in that Operation, executed in the Year of the Republic 11*; by* ANTONY THILLAYE-PLATEL, *House Apothecary at the Hotel-Dieu at Paris* †.

THE idea of the experiments, of which I am going to give an account, was suggested to me by the discovery of thermolamps.

* Perhaps this discovery of Mr. Vauquelin may account for the two ancient candlesticks in the cathedral of Hildesheim, in Lower Saxony; made we believe long before any platina could be brought from South America, though we do not know their exact date; and mentioned by Professor Cramer, of that place, in his Letters on Natural Philosophy. These are described as white, and nearly as heavy as gold, and probably therefore consist of such an alloy as would be obtained from a portion of the ore of Guadalcanal, rich in platina and poor in silver; and which Bishop Bernward, their maker, though one of the most skilful metallurgists of his time, did not know how to separate. T.

Ancient candlesticks probably from the same, or a similar ore.

† Annales de Chimie, Vol. LVIII. p. 128, May, 1806.

I was

Manufactory for charred turf.

I was at Rouen, employed at the hospital under Mr. Robert, chief apothecary, a man for whose talents I have the highest respect, on more accounts than one; and he was repeating some experiments relative to these discoveries, which when I saw, I conceived the idea, that the apparatus might be employed for more carbonizing processes than one; and I communicated to him my thoughts respecting some questions, that had been put to me, on the possibility of converting turf into a charcoal, capable of being substituted for that wood.

It had been proposed to me, to form an establishment capable of manufacturing a very large quantity at a time. Mr. Robert approved my scheme; and, assisted by his judicious advice, I undertook a manufactory of turf charcoal some miles from Gournay.

I had already obtained some success, when circumstances foreign to the business occasioned it to fall to the ground, and ruined an undertaking on which I had long rested all my hopes.

Though I here bring forward new methods, it does not follow, that the product of some manufactories, among others those of Meaux near Paris, are not of good quality, as the public begin to be sensible.

Turf first pressed.

In order that the turf may present the greatest possible substance in a given bulk, I expose it to a regular continued pressure; by which means it quickly loses all the water it contains, its desiccation in the air is more speedy, and thus we gain the advantage of a saving of time.

Mode of placing it to be charred.

After this pressure, though the charring might be performed without this preliminary operation, care is taken to place the turfs so, that the masses they form shall present demiobstructions to the air, to accelerate its currents.

In this state it is subjected to carbonization by means of an apparatus, which will be described below.

Products by distillation.

Observation having proved, that vegetable substances afford advantages even in their distilled products, I concluded, that the oily and condensable matters should be separated from the gasses, which I intended to employ as a supplementary support of the combustion.

The gasses supply fuel.

This observation is so much the more valuable, as these very

very gases may supply the place of one fourth of the turf or combustible employed in the carbonizing fire, an advantage hitherto neglected.

I would beg leave here to remind the reader of some facts relative to the order in which the gasses are disengaged, during the action of caloric applied to vegetable substances.

Effect of caloric.

It is known that caloric, in contact with these vegetable substances, disorganizes them wholly or in part; that it solicits their three remote principles to combine according to their various affinities, and at different temperatures; and that the results are products very different from the original compound.

First products, water, oil, and acid;

Thus, for example, the most volatile substances, or those the principles of which have the strongest attraction for each other at a low temperature, are first disengaged: the water, oil, and vegetable acid, pass over first, whether they were partly contained in the vegetable substance, or that their principles were induced to combine by the predisposing affinity of caloric: but at a high temperature, at a red heat, carbon decomposes water, this ceases to be formed, and the carbonic acid passes over, with carburetted hidrogen surcharged with carbon, and oxidule of carbon; the fixed substances remain in the distilling apparatus; and, if azote be contained in these substances, it is at this period the carbonate of ammonia is disengaged.

then carbonic acid, and carburetted hidrogen.

Carbonization of large quantities different.

Though the phenomena take place in this manner, in small masses, heated equally in all their parts, it is not the case with several hundred weight of materials, the outside of which will be carbonized, while the centre of the mass has scarcely experienced the effect of the caloric acting in the inverse ratio of the square of the distance.

Proportions of the products gradually differ.

Accordingly we may expect to find the products differing in their proportions at different periods: then the water, oil, and acid, will predominate at first, and will subsequently decrease in their proportions, and be more carbonized.

Empyreumatic oil.

We shall then find a black, oily, acrid substance appear, more or less heavy, which is the empyreumatic oil, and indicates a carbonization approaching its end in a favourable manner.

The

Charcoal. The last result is a fixed black substance, tolerably homogeneous, and weighing more than an equal bulk of charred wood. Frequently, in consequence of the sand, the oxide of iron, and the compactness acquired by the previous compression, this substance is the true charcoal of turf; which sometimes, before it is obtained, furnishes a certain quantity of sulphurous acid, arising from the combustion of the sulphur and sulphate of iron contained in such turf as I have dug. This justifies to a certain degree the complaints of persons, who refuse to make use of this combustible; but this slight defect may be removed by very easy means, which I employ in burning turf in rooms, and of which I shall give the particulars hereafter.

I shall now give a description of my apparatus, which I shall divide into two parts; the first describing the furnace, the second the interior part, which I call *thermolampic*.

The furnace described. The furnace is square, terminating above in an arch; and in the front appear three apertures, one over another.

The ash-hole. Its inferior part, in which is the ash-hole, is shaped interiorly like a wedge, the base of which is the aperture, one of the square sides lying uppermost and horizontally, the other, an inclined plane, forming the bottom. This form prevents any ashes from lodging in it to obstruct the fire, and renders the current of air more rapid. It is obvious that the upper part of this ash-hole is formed by the grate, which consists of movable bars of iron arranged parallel to each other by means of a cross piece. This arrangement facilitates the arrival of the air, and accelerates the combustion. Above the ash-hole is the second opening, which is that of the fire-place, and is carefully closed with a large stone shod with iron, and furnished with two rings, to admit a hook, by which the door is removed whenever fuel is to be thrown in. See Fig. 2. Plate III.

The fire-place.

Chimney. The third aperture is perceptible only by the projecting part of the bottom of the chimney, which suggests that the smoke is obliged to surround the interior apparatus: and this is in fact the case, since it returns to find an exit exactly on a level with the bottom of the thermolamp, which is supported by the interior and anterior part of the furnace.

By

By the chimney it may be observed, that the superior aperture is less than the inferior, which is indispensable in this construction; and this leads me to say, that there always exists a direct ratio between the apertures of the fire-place, the ash-hole, the place where the smoke enters into the chimney, and that at which it finds its exit, which should always be proportional to the height. Thus ash-holes of a moderate depth and aperture, fire-places narrow and well closed, turns (*chicanes*) artfully managed, and a convenient issue for the smoke, all united constitute a furnace, the good qualities of which are demonstrated by experience. See Fig. 1.

The second part of my apparatus, which I call thermolampic, is so arranged, that its lower part is horizontal, and forms a long square. The upper part of its whole length is an elliptical arch, terminated at each extremity by a vertical plane, in the middle of which is a funnel or tube bent at a right angle, serving to convey the products that arise by distillation into the middle of a condenser, consisting either of a stone hollowed out, and covered by a plank well luted to it, of a small wooden cask standing upright, or of a cast iron tube surrounded by a stream of water. From these issue tubes to convey the inflammable gasses into the fire-place, that they may serve, as I mentioned above, instead of part of the fuel employed. The funnels above described are furnished each with a key, to intercept the communication between the outer air and the charcoal, while still hot: for experience has proved, that charcoal thus prepared is capable of spontaneous accension. This phenomenon, it is said, may take place in large heaps of charcoal long prepared; and, though I confess I never saw such an accident, it is certainly prudent to guard against it by currents of cold air traversing the heap in various directions. See Fig. 3.

Thermolamp.

Condenser.

Gas pipes.

Charcoal kindled spontaneously.

The material of which the thermolampic apparatus is composed should be sheet iron, or thin cast iron; though one or more common cylinders may be substituted instead, placed in a suitable manner, and furnished at their extremities with tubes for conveying off the gaseous products, &c.; taking care that one end may be opened by means of a hook,

Materials.

Opening for the substance to be charred.

a hook, and closed with a proper lute. To all these must be added, that the part of the furnace, through which the substance to be charred is introduced, should be made only of dry bricks, and covered with wet clay.

Position of the apparatus.

Whatever be the form or material of the thermolamp, it ought always to be placed horizontally in the furnace, and have its extremities resting on the side walls. In this situation the bottom and circumference will receive the action of the fire, which must be fed with the bad turf, as I have mentioned, assisted by the gas from the lateral tubes.

Bar for security.

To all this must be added a bar of iron, in the direction of the width of the apparatus, to prevent it from giving way when loaded, and long exposed to heat.

Turf dried by the heat of the smoke.

I have turned the heat arising from the smoke to great advantage, by constructing a kind of stove, to prepare the turf for the charring apparatus. The most suitable means for this drying process, particularly in winter, may readily be conceived.

Sixty pounds of wood charred.

Into an apparatus disposed as I have described, I put upward of sixty pounds of dry wood, that I might form an accurate idea of its advantages; and the following were the results.

Results of the process.

After having commenced the extrication of the gasses with turf of good quality, acquired by pressure, they burned with vehemence, and at the expiration of an hour furnished such a quantity of radiant heat, that the tubes alone, without any addition of fuel, were capable of continuing the operation, affording in the conclusion a perfectly homogeneous charcoal; and I confess, that I never saw what appeared to me a more beautiful sight. How indeed can we behold without admiration a combustible burning itself, and thus saving half the fuel, that would have been required to convert it into charcoal?

Domestic use of pressed turf.

I must not here omit to speak of the use of pressed turf for domestic purposes. For instance, I have found by experience, that turf of a good quality, after having been pressed and well dried, produced heat in the following proportions,

Compared with wood

Five or six parts of turf are equal to four of wood, supposing the fireplace to be extremely accessible to air. There

is

is a great advantage in burning turf therefore, supposing it even not to be pressed, and that in this case it would require two, three, or even four parts to one of wood; for, if we consider the value of both, we shall find, that for the same price three times as much turf as wood may be burned.

Objections to it.

But the public are unwilling to adopt this economical practice, alleging, that turf emits a disagreeable smell *, and gives but little heat.

Not valid in English fire-places.

All these errors would be dissipated, if fire places were constructed nearly like those used in England for burning coal or coak; and I may add, that I have had the pleasure of seeing persons, who were greatly averse to innovation, rejoice at having made trial of a fire place of my construction, for the use of a species of a fuel on which they would scarcely deign to cast an eye. See Pl. IV. figs. 4, 5, 6.

Proportions of charcoal, and of ashes, produced from turf;

Turf does not always afford an equal quantity of charcoal, and of course the quantity of ashes must vary in the same proportions. I have seen turf that afforded 0·38 or 0·40 of charcoal, which left on incineration 0·17 or 0·18 of ashes. I do not mention certain kinds, that have produced 0·50 of ashes, since from these must be subtracted 0·35 of ferruginous sand, which they contained.

The turf that I employed in my manufactory produced, after a well managed process, from 0·38 to 0·42 of charcoal, and yielded from 0·13 to 0·16 of ashes; but I am inclined to think, that, when the saving occasioned by the use of the gases is considered, the quantity of charcoal may be raised by secondary improvements to 0·50. The quantity of ashes produced by turf charcoal will be thought very considerable, when compared with the quantity arising from sound wood. Thus on examining oak freed from its alburnum, and of fifty years growth, we find that fifty parts yield from twenty to twenty-one of charcoal, and from two to two and a half of ashes.

and from oak.

Turf may be used in making bricks, tiles, or earthen ware.

Finally I will add, that pressed turf, or turf of a very good quality, may be used in burning bricks or tiles; and in baking common earthen-ware, &c. for three fourths of

* This smell is produced only by unprepared turf.

the process, finishing it with one fourth of well burning wood; and this earthen ware will be equal to what is commonly made for domestic purposes, as I have found by experience.

To obtain such results, I have myself constructed a furnace on the principles laid down above. Neither was the preparation of the earth for each kind of pottery forgotten: this was the basis of my labour.*

Oily products.

I have already observed, that the oily products received in the condenser might be turned to advantage. These products are frequently divided into two strata: the first water, impregnated with a small quantity of a light oil, and containing acetous or acetic acid: the second a black, heavy, acrid, very penetrating oil, thick like tar, and difficult of solution. This may be used for various purposes.

Acid useful for making iron-liquor, for dyers or calico printers;

The rectification of these oils furnishes a certain quantity of acid, by means of which I have prepared the solution of iron, called in manufactories iron liquor; but it is proper to add a little concentrated acetous acid, in order to prevent the precipitation of the iron in the state of oxide.—With a solution of this sort I have prepared by a particular method patterns of black, on cotton, silk, and woollen, which were not inferior to those dyed in the usual way. I could likewise produce very good nankins; and the buffs on printed calicoes might be prepared with iron dissolved in this acid, thickening the composition with starch or gum, according to the value of the article and the tint required.

What I have said respecting iron dissolved in the acetous acid may be extended to the employment of the empyreumatic oil in dyeing wool, &c. and a number of other processes, too many to enumerate.

and manufacturing verdigris.

Lastly, I am persuaded, that it is possible to oxide copper by means of this acid, which however is afforded in smaller quantity perhaps by turf than by wood.

Improvement in tiles.

* I would beg leave to mention an improvement I proposed in tiles, and which I carried into execution. This consists in making a tongue to them of a triangular shape, the base being very large and at bottom; and with respect to the moulding, it may be sufficient to say, that two men could do the work of four in a given time.

To

To produce that preparation of copper, which is called verdigris, the remains of certain cider apples might be used, after the juice has been pressed out, of which there is a great deal throughout Normandy, that is turned to no account, except as a bad kind of fuel. In this case it should be moistened with bad sour cider immediately after it is taken from the press, and put into earthen pots, or little casks, with plates of copper, *stratum super stratum;* washing the oxided copper with our acid, after it has thus stood a month, and then proceeding according to the method of Mr. Chaptal. Various wild fruits, as the sloe for example, might be used in a similar manner, instead of the refuse of the cider press.

The pressed pulp of cider apples applicable to this purpose,

and many wild fruits.

I am indebted to Mr. B. E. Lefébure, my friend and countryman, and a zealous cultivator of chemistry, for the first idea of employing the pressed pulp of apples in preparing the acetate of copper.

The facts related in this paper will prove interesting I hope to science, and of some utility to the public; and if I meet with the reader's indulgence, my wishes will be gratified.*

Explanation of the Figures.

Explanation of Plate III.

Plate III. *Fig.* 1. an interior view of the furnace.

Q, Q′, upper part of the furnace.

M, M′, the chimney, the shape of which may be varied.

H, I′, iron hoops, keyed } to strengthen the apparatus.
B, B′, B″, B‴, other hoops }

L, L, little air holes, to accelerate the combustion at pleasure.

q′, the fire-place closed by a stone with two rings, to admit the introduction of the hook R, fig. 2.

N, four iron bars forming the grate.

D D′, an iron bar, fastened into the brick work at each end, upon which the bars N slide.

* The day on which my memoir was received, I was informed by Mr. Vauquelin, that Mr. Lebon, engineer of bridges and highways, inventor of the thermolamps, had applied processes nearly similar to mine to the carbonization of wood in his experiments. I conceive therefore it is but justice to say with Mr. Vauquelin, that nothing can be better contrived, than the apparatus of Mr. Lebon.

K, the ash-hole, nearly in the form of a wedge placed horizontally.

Fig. 2. A, a, tubes to convey the gases from the thermolamp into the condenser.

E, e, keys to cut off the access of the external air to the charcoal, while yet hot.

C, c, tubes to convey the gases that are not condensible into the fire place.

F, f, condensers to be employed at pleasure. F is a stone hollowed out, and properly covered: f, a tub, or, if it be preferred, a small cask.

S, S′, the pipe for the discharge of the distilled fluids.

Fig. 3. The carbonizing or thermolampic apparatus.

A, a, tubes issuing out of the furnace.

B, a crook to keep in its place the part opening at the side, where the masonry forms a door closed with dry bricks, removed and replaced at every operation.

C, body of the apparatus.

The dotted lines express the parts that are concealed, and are continuations of the lines drawn full.

Plate IV. *Plate* IV. *Fig.* 4. Interior view of a fire place for burning turf, or turf charcoal. By this construction it appears, that the disagreeable effects ascribed to the burning of turf are avoided.

A, mantletree of the fire place.

B, a plate of metal, rising and falling in two lateral grooves by means of the two copper buttons, C, C′.

E, the back, formed of a plate of cast iron, sloping at the upper part.

D, D′, the mantle-piece.

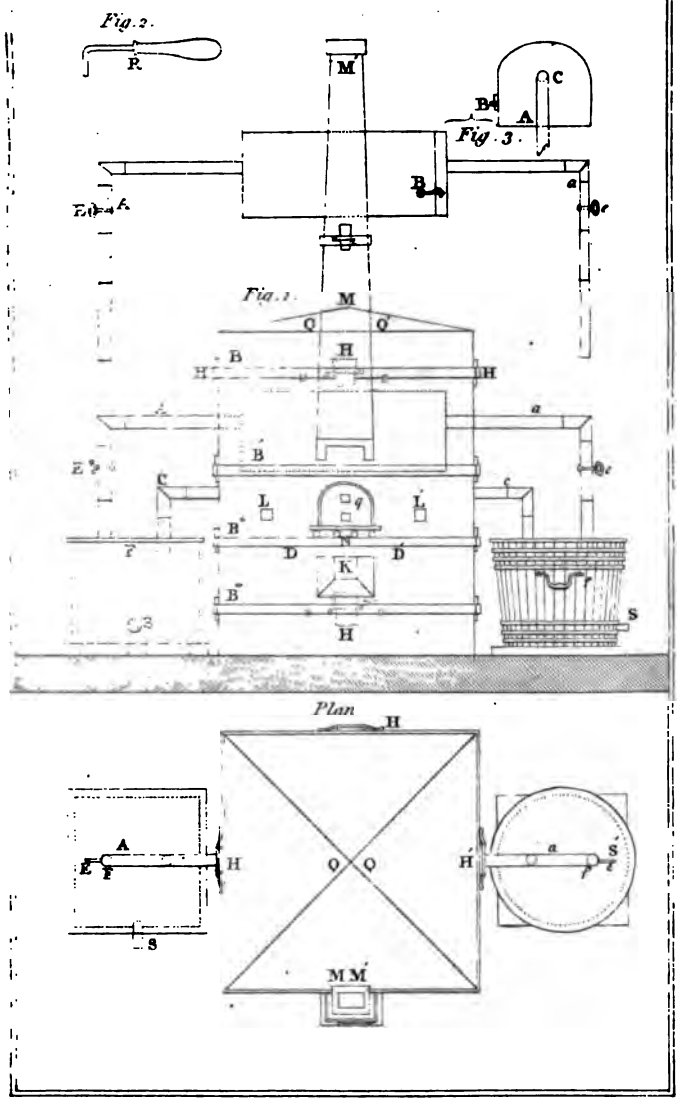
F, F, the sides, forming an angle of 135° with the bottom.

G, a kind of box, consisting of two grates; the interior one forming a pretty open angle with the bottom of the fire place; the other, making the fore part of the box, and consisting of two or three parallel bars. By this arrangement the turf, which requires only a very rapid current of air to burn it, is isolated.

H, the length of the bottom of the box, formed like the other part of common cast iron.

I, the

Apparatus of M. Thillaye Platel, for the Carbonisation of Turf.



I, the ash-pit, the bottom of which is a little arched toward the farther part, so as to give it greater depth.

K, K′, K″, the castors of the ash-pit.

Fig. 5. The profile or vertical section of the fire place.

A, the mantletree.

B, the anterior plate or regulator.

E, D, the posterior plate, of cast iron, curved so as to leave an opening for the smoke four inches broad by sixteen or at least fourteen long.

It must be observed, that this plate does not reach quite to the top, and that the line is continued by loose bricks, to facilitate the passage of the chimney sweeper; in addition the interval E is filled with a mixture of powdered charcoal and mortar or clay.

G, shews the shape of the grate into which the turf or charcoal is put.

Fig. 6. P, a pair of cranesbill tongs.

VI.

Method of curing Damp Walls, by the Application of a Composition newly invented by Mr. CHARLES WILSON, *of Worcester Street, near Union Hall, in the Borough.**

SIR,

I BEG leave to lay before the Society of Arts, &c. a cement, which, I trust, will be found of great utility in curing damp walls, in flooring damp kitchens, and for various other purposes, where the prevention of wet is necessary. Cement to keep out wet from walls or floors,

This cement when put in water will suffer neither an increase nor diminution in its weight: and it has the peculiar advantage of joining Portland stone, or marble, so as to make them as durable as they were prior to the fracture. and join stone or marble.

I have the honour to be,

Your very humble servant,

CHARLES WILSON.

* From the Transactions of the Society of Arts, who voted a premium of ten guineas to the inventor.

Receipt

Receipt for making the Cement.

The cement. Boil two quarts of tar with two ounces of kitchen grease, for a quarter of an hour, in an iron pot. Add some of this tar to a mixture of slaked lime, and powdered glass, which have passed through a flour sieve, and been dried completely over the fire in an iron pot, in the proportion of two parts of lime, and one of glass, till the mixture becomes of the consistence of thin plaster.

The cement must be used immediately after being mixed, and therefore it is proper not to mix more of it at a time than will coat one square foot of wall, since it quickly becomes too hard for use, and continues to increase its hardness for three weeks. Great care must also be taken to prevent any moisture from mixing with the cement.

For a wall which is merely damp, it will be sufficient to lay on one coating of the cement, about one eighth of an inch thick; but should the wall be more than damp, or wet, it will be necessary to coat it a second time.

Plaster made of lime, hair, and plaster of Paris, may be afterwards laid on the cement.

Mrs. Ann Kemmish, King Street, Borough; Mr. Boone, Gregory Place; and Mr. Thomas Cannadine, Hook's Gardens, Tooley Street, have certified that Mr. Wilson's cement has been used with effect, on damp walls belonging to them.

VII.

Experiments on the Effects produced by a High Temperature on the Animal Economy. By F. F. Delaroche, *of Geneva.**

Living beings have a peculiar power of resisting cold, AMONG the numerous characters that distinguish organized bodies, and particularly those of animals, from inanimate substances, one of the most remarkable beyond question is the faculty they have of resisting cold, and preserving in general a temperature superior to that of the medium in which they are placed. Accordingly this property of

* Journal de Physique, Vol. LXIII. p. 207. Sept. 1806.

living

living bodies has attracted the attention of physiologists in all ages, who have invented a thousand hypotheses, more or less probable, to account for it.

and also heat.

It has not been the same with the faculty enjoyed by animals, and perhaps by plants likewise, of resisting heat, and preserving a temperature inferior to that of the circumambient medium. Scarcely any researches on this head were made previous to the eighteenth century, when the invention of thermometers had enabled the philosopher to measure the heat of bodies with accuracy. The first experiments that were attempted might have led us to doubt the existence of such a faculty. Fahrenheit and Provoost, at the suggestion of Boerhaave, exposed three animals in a sugar baker's oven, the temperature of which was 146° F. One of these animals was a dog weighing 10lbs. one a cat, and the third a sparrow. All these died, the cat at the expiration of seven minutes, the other two in twenty-eight. These experiments were undertaken to verify a theory of Boerhaave's respecting the use of respiration. He had supposed, that it served, by the access of fresh air, to cool the lungs; in which, according to him, the blood underwent a fermentation, that produced a very considerable degree of heat. From the result of this experiment he thought himself authorized to conclude, that his theory was well founded, and that no animal could live exposed to a heat higher than its own temperature.

First experiments. Dog, cat, and sparrow, killed by a heat of 146°, in 7 or 28 minutes.

Boerhaave's theory of respiration.

The opinion of Boerhaave seems to have been generally adopted for a certain time by physiologists. It does not appear, that any precise notions of the temperature of hot climates were entertained at that time; but afterward more accurate ideas of it were formed, which did not agree with the law established by Boerhaave. In 1748, Dr. John Linings of Charlestown, giving an account of the meterological observations he had made in that place, noted the high temperature observed there in summer. Fahrenheit's thermometer in the shade frequently rose to 85° or 90°; and once he saw it as high as 98°. Though he did not examine the temperature of places exposed to the sun, he estimated with much probability, from other observations made in lower temperatures, that it must have been 124°. Adan-

Temperature of hot climates.

At Charlestown 85°, 90°, and 98°. In the sun 124°.

son, in his account of his voyage to Senegal, makes some observations on the heat he had experienced in that country. Among other facts he relates, that, in an excursion he made in a small vessel on the Niger, the temperature of the cabin in which he remained was from 115° to 125°, and did not fall below 99° during the night. In 1758, Mr. Henry Ellis, governor of Georgia, communicated to the Royal Society a fact respecing the excessive heat he experienced that year at Savannah. The thermometer in an open room facing the north rose to 102°. He likewise says, that going abroad with an umbrella, to screen him from the sun, a thermometer, which he held in his hand, rose to 105°: and that the same thermometer, when applied to his body, to his great surprise fell to 97°.

In Senegal, 115° or 125°, and at night 99°.

In Georgia 102°, and 105°, yet that of the body only 97°.

Observations respecting the temperature of vapour baths contributed likewise to shew, that man can support the action of a temperature superior to that of his own body. Such are those of Gmelin*, who observed, that the heat of the Russian vapour baths rose to 108°, and even 116° F.

Russian vapour baths, from 108° to 116°.

Some experiments on animals by Arnold Duntze† afforded similar results with respect to them. Dogs confined in a stove were capable of supporting a temperature of 106°, or even 108°, for a considerable time. It is true, however, they died, when the heat was raised to 113° or upward.

Dogs supported 106° or 108°, but killed at 113°.

Haller, in the second volume of his Elements of Physiology, has collected these and other similar facts, from which he concludes, that both men and animals, under certain circumstances, can support a temperature superior to that of their own body; adding, that in one or two cases the persons, who had observed this fact in themselves, had likewise remarked, that their own temperature kept itself below that of the surrounding medium.

Animals then support high temperatures, and their bodies are colder than the air around them.

In 1760 Tillet and Duhamel had an opportunity of seeing at Rochefoucaut in Angoumois a baker's maid-servant go into an oven, the temperature of which was at least 264°, and stay there about twelve minutes, without much incon-

A girl could stay 12 minutes in an oven at 264°.

* Flora Sibirica, t. I. pref. p. 81.

† Arnoldus Duntze, Experimenta Calorem Animalium spectantia, Leyden, 1754. Quoted by Haller.

venience.

venience. After they were gone, a person who had been present at this experiment repeated it several times at their request with another girl, employed in attending the same oven, and the results were the same. It is to be observed, that a spirit thermometer was used on this occasion, which gave the temperature of the oven only by approximation. Tillet considered the result of these experiments as militating against that of Boerhaave's. It appeared to him astonishing, that animals should have been destroyed in so short a time by a temperature of 146°, while women could support a temperature of 264°: and he inferred, that the speediness of the death of those animals must be ascribed to some cause foreign to the heat, such as the vitiation of the air in which they were included. In consequence he made some experiments, to ascertain how far Boerhaave's opinion was well founded; who, in consequence of his theory of the use of respiration, attributed the fatal effects of the heat to its action on the lungs alone. He exposed some animals in an oven heated to 156° or 166°. First he put them in naked, and let them remain some time: then, having taken them out, and allowed them time to recover themselves, he wrapped them up in linen cloths, which covered the whole of their body, and put them in again. In the latter state they supported the heat much better than in the former. Hence he concluded, that the heat does not act on the organs of repiration alone, but has a general effect on the whole body.

Another did the same repeatedly.

Tillet's remark.

Exposed animals to 156° or 166°, which they bore, but much better when clothed.

Franklin, in a letter which he wrote to Dr. Linings, published in the Journal de Physique for 1773, after giving an account of the researches he made in respect to the refrigeration produced by the evaporation of fluids, endeavoured to explain by this property a fact, which he had formerly observed in himself. On a summer's day, the temperature of the air being 100°, he had remarked that his own temperature was only 96°. He was at the time lightly clothed, and in a profuse perspiration. The reason of this difference of temperature he imagined therefore, to be the evaporation going on from the surface of his body.

Franklin observed the heat of his body less than that of the air,

and ascribed it to evaporation.

In 1775, Dr. Fordyce joined with Sir Joseph Banks, Sir Charles Blagden, Dr. Solander, and some other natural philosophers,

Experiments by Dr. Fordyce, &c.

losophers, to make fresh researches into the influence, that high temperatures have on the animal economy. Their experiments are too well known, to be repeated here: it is sufficient to say, that they could support for several minutes, without being too seriously inconvenienced, a heat superior to that of boiling water; and that they confirmed, in a more accurate manner than had before been done, the faculty man enjoys of keeping himself at a nearly constant temperature, though placed in an atmosphere of which the heat is far superior to his own. These gentlemen, struck with the copious perspiration, that was formed when they were exposed to the heat; observing too, that the moment when this perspiration shewed itself was distinguished by a diminution of the painful sensation they experienced from the heat; were led to suppose, that the evaporation from the surface of the body contributed greatly to this uniformity of temperature. Some experiments they made on the heating of liquids exposed in open vessels, and introduced into the heated room, confirmed them in this opinion. In fact these liquids kept themselves uniformly at a temperature below that of the surrounding medium, and could not be brought to boil, till they were covered by a stratum of melted wax, which prevented the evaporation. Nevertheless these gentlemen did not think, that evaporation of the perspirable matter was the sole cause of the uniformity of temperature, which they had observed in themselves, though exposed to a heat so much higher.

Supported a heat above 212° some time.

Ascribed it to evaporation,

which appeared to keep liquids under similar circumstances from boiling.

They supposed however some other cause.

Dr. Dobson made similar experiments.

At the same time Dr. Dobson, of Liverpool, made some experiments in the hospital there, which were nearly similar and attended with similar results.

J. Hunter applied heat to parts of the body,

About the same period too, or a little after, Mr. John Hunter published some inquiries he had made respecting the heat of animals. Most of these related to their faculty of enduring cold; some however respected their capacity for resisting heat. The latter were not made on the whole body of men or animals subjected to the experiment, but on particular parts merely; and Mr. Hunter thought he perceived, that this faculty, though it could not be considered as absolute, was more decidedly marked than that of resisting cold.

and imagined its effects were resisted better than those of cold.

When Sir C. Blagden's first paper appeared, Mr. Changeux made some remarks on it.* He particularly endeavoured to prove, that it was not by virtue of a particular property, that the human body resisted the effects of heat, but from causes purely physical. These causes, according to him, were on the one hand the evaporation of the perspirable matter; on the other the refrigeration of the air introduced into the lungs, the effects of its rapid passage through the trachea. He does not appear however, to have made any experiment on the subject.

Changeux ascribed it partly to evaporation,

partly to the cooling of the air in its passage to the lungs.

In 1779, Dr. Crawford, in the first edition of his work on animal heat, promulgated the opinion, that the faculty possessed by animals of producing cold depended solely on the evaporation of the perspirable matter pulmonary and cutaneous. Subsequently, in a paper in the Philosophical Transactions, and in the second edition of his work, he advanced a contrary opinion, founded on some experiments of which I shall give an account in another place.

Dr. Crawford, at first to evaporation solely from the skin and lungs,

afterward in part to less oxigen being absorbed from the air.

Having observed, that animals exposed to heat vitiated the air less by respiration than such as were exposed to cold, he thought he could explain by this fact the faculty of producing cold which they possess. I shall not attempt here to give an account of the theory he invented on this point, a theory which I confess I do not very well understand.

Such are the principal researches and observations, that, to the best of my knowledge, have been published respecting the influence of heat on animals. The subject however was far from exhausted, as several questions remained undecided, and others even wholly neglected. A few inquiries, however incomplete, that I have made myself respecting it, will form the conclusion of this essay. They are far from filling up the chasms that were left; but I shall deem myself happy, if they throw some light on a few points, and meet the indulgence of the enlightened judges, to whom I submit them.

It is incumbent on me to add, that the experiments, which constitute the base of these researches, are not exclusively my own; they were made in concert with my friend, Dr.

* Journal de Physique, t. VII. p. 57.

Berger,

Berger, of Geneva, who shared the labour, and assisted me with his advice.

SECT. I. *Of the Degree of Heat that Animals can endure.*

The limits of this faculty not determinable with precision.

It is scarcely possible to investigate the effects produced on animals by heat, and the faculty they have of resisting it, without being prompted to ask, what are the limits of this faculty? in other words, what is the greatest degree of heat they can support, without being deprived of life?—This question however is insusceptible of a precise answer.

The time must be considered as well as the intensity.

The effects of heat being in the ratio of the duration as well as intensity of its action, it is not till a very long time has expired, that we can consider an animal as having undergone all the influence of the heat to which it has been exposed, and conclude it to be capable of resisting it.

The temperature subject to variation,

We cannot likewise prevent this temperature, on the one hand, from experiencing considerable variations, which hinder its being ascertained with precision; and the animal, on the other, from being subjected to the influence of foreign circumstances, by which the effects of the heat will be modified.

and the animal affected by various circumstances.

Conclusions from the author's experiments.

The author here relates various experiments made on several animals, from which he draws the following conclusions.

From these experiments it follows, as might have been presumed, that all animals are not equally affected by heat, and that the faculty of resisting it is not the same in every species. We cannot therefore derive from them any general and precise conclusion with respect to the measure of this faculty. These experiments however are sufficient to shew, that most animals, at least those of a small size, sink under a temperature of 144°, or even 134°, after a certain space of time, which is generally pretty short. They shew too, that the progress of the symptoms is more rapid, and the arrival of death more speedy, in proportion as the heat is greater.

Small animals killed by a heat of 134° or 144°.

The larger the animal the better it resists heat.

The size of animals appears to have a marked influence on the speediness of the effects of heat. The ass supported them much longer than the cat, the dog, the rabbit, and the guinea-pig; and these longer than the mouse. The magpie

magpie and the bunting were killed sooner than the cock or the pigeon. The difference was scarcely less striking between a large and a small frog, a beetle (*scarabæus nasicornis*), and a wood-louse. It was not the same however in all cases. The guinea-pig, though less, appeared to support heat a little better than a rabbit exposed to the same temperature; and the sparrow lived longer than the cock and the pigeon. Exceptions.

The results were not less modified by difference of organization. Frogs and cold blooded animals supported heat much better in proportion to their size than hot blooded animals. The larvæ of beetles, leeches, and fresh-water snails (*bullæ fontinales*), though still smaller, supported it equally well. It was not the same with beetles in their perfect state, mole-crickets, and wood-lice, which were killed much more quickly. Cold blooded animals endure heat best. Other exceptions.

(To be continued.)

VIII.

On the Increase of Temperature produced by the Galvanic Action. By Mr. John Tatum, *Jun.*

To Mr. NICHOLSON.

Dear Sir,

In the paper I sent you about two or three months ago, on the rise of the temperature of water during its decomposition by galvanism, I proposed sending you the results of other galvanic experiments I had made near a twelvemonth since: but having mislaid the minutes I took during the experiments, and various avocations preventing my repeating these until the present time, is the cause of my not fulfilling my intention so soon as I wished. Additional experiments on galvanism.

In the following experiment I had two objects in view, the one was, to ascertain the temperature to which the water rose during its decomposition: the second, to confirm the faint recollection I had of muriatic acid being formed in the experiment alluded to: for which purpose I made use of two troughs, each 26 plates, each plate 50 inches surface; and two

two troughs, each 25 plates, each plate 36 inches surface; with diluted nitrous acid I had made use of four days before, now adding a little more acid.

Description of the apparatus.

As the apparatus I made use of to contain the water is one of my forming and making, it will perhaps be necessary to describe it, before I relate the result of the experiment.

Let A B C D, *Pl.* IV. *Fig.* 7. represent a glass tube capable of containing 1½oz. of distilled water; E F a brass cap, through which passes a screw G, to which can be attached a platina or other wire; and by taking the screw out another sort of wire may be fixed, as is represented by O: H a box with leather through which the thermometer I passes, and then screws on tight, graduated on the tube; K L a basin, in which the tube rests when filled with distilled water; M a neck, which may be fitted in any convenient place to support the apparatus, screwing through the basin, and terminating in a pair of forceps, into which may be inserted any sort of wire.

On forming a connection at G with one end of the battery, and at M with the other end, the galvanic fluid will pass through, and decompose the water in the tube; and the thermometer will indicate the temperature.

Having explained the apparatus, I proceed to the experiment.

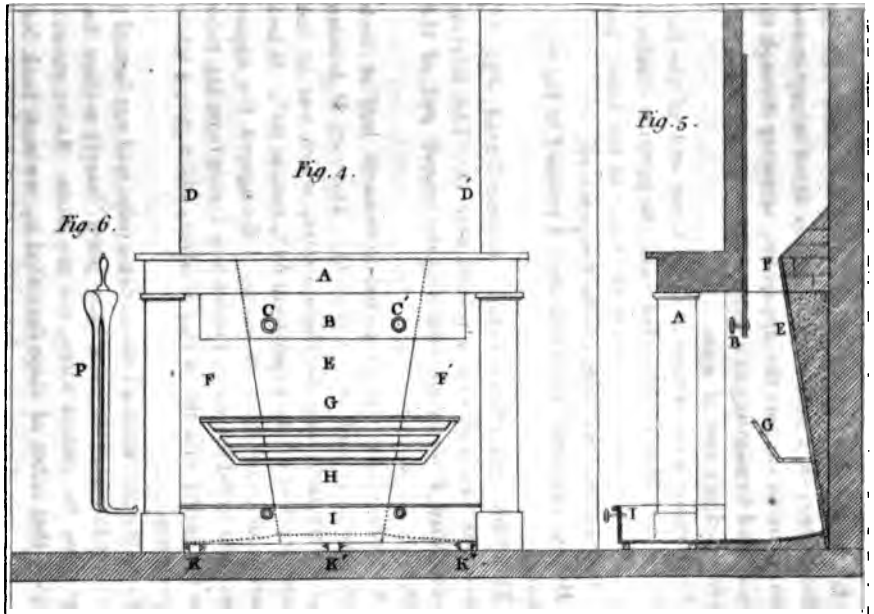
The zink end of my battery communicated with G, which was provided with a platina wire O. The inferior platina wire P was connected with the copper end of the battery.

Elevation of temperature 26 degrees.

A stream of gas was projected downwards half an inch from the lower end of the wire O. The wire P became oxided very fast; and the thermometer, which was at the commencement of the experiment 54°, rose to 80°. When I had decomposed 1½oz. of water, I disengaged the apparatus, and tested the water (which was forced from the tube into the basin) with the nitrate of silver, which gave a white appearance.

Muriatic acid.

From this appearance I conclude muriatic acid was formed; but what furnishes its constituent parts? surely neither the glass tube, the platina wires, nor the basin. As we cannot suppose that either of these furnished it, we must look for its



Apparatus of Mr Thillaye Platel for the Carbonisation of Turf

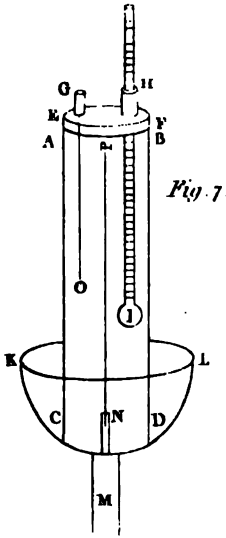


Fig. 7.

Mr Tatum's Galvanic Apparatus, p. 150.

its component parts in the distilled water, which is well known to be composed of oxigen and hidrogen; and oxigen being considered as the acidifying principle, I conceive, that the muriate formed is an oxide of hidrogen, but in such proportions as constitute an acid.

In hazarding this opinion I know I differ from, I believe I may say, all of the most reputable modern chemists, such as Thomson, Fourcroy, Accum, &c. who, when speaking of acids, say 'the base of muriatic acid unknown'; and also, that 'oxigen forms no other combination with hidrogen, than that which constitutes water'. I hold these authors in the highest respect, and derive the greatest information from their works, which I conceive do them great credit; but facts are stubborn things, and I make it a rule, to bow my theory to truth.

I am, Dear Sir,

Your most obedient,

JOHN TATUM, *Jun.*

April 14, 1807,
Dorset Street, Fleet Street.

P.S. At first I expected the mercury in the thermometer to rise much higher, but, owing to using the diluted acid a second time, the water was decomposed much slower than in the experiment in my former paper; this, added to the apparatus being considerably larger, much more metal about the cap, and the thermometer having a largish bulb, all of which either absorb or conduct off the caloric, will account for my disappointment.

SCIENTIFIC NEWS.

On the Tempest of Feb. 18, *which has produced many dreadful accidents in the Channel.*

(Continued from p. 88.)

No one can read the detail of the numerous shipwrecks which are mentioned in letters from Havre, Dunkirk, Dieppe, St. Valery, and Calais, without being deeply afflicted. Further details from Laon, Bruges, Ghent, and Paris, augment the melancholy

Observations by la Marck on his system concerning the influence of the moon upon the weather.

melancholy list of incidents. Surely, it is high time, that the causes which produce such dreadful events were taken into serious consideration, and that an inquiry into them should receive the attention and interest to which it is entitled.

Paris, Feb. 25, 1807. LAMARCK.

The following letter from M. de Lalande to the editor of the *Moniteur* has been inserted in that paper of the 1st March 1807:

Remarks on the same by La Lande.

It does not appear in any wise probable to me, that the passing of the moon through its *nodes* produces any sensible change in the atmosphere, as M. De Lamarck thinks: but its passing over the equator is more observable; I have noticed it many times; and even this year, in the months of January and February, there have been alternations of cold and heat, which appeared to follow the passings of the moon over the equator. For that reason, I have marked them in the annuary of the Board of Longitude, from the beginning.

But the dreadful hurricane of the 18th February can have no relation to the moon. These phenomena proceed from the winds, from thunder, and from volcanoes, or swellings of the sea. We may hereafter learn, perhaps, that on the 18th February, there have been violent thunder storms in some of the southern provinces, and I should wish to be informed of it through the *Moniteur*, a paper in which scientific men like to deposit their observations and remarks.

(Signed) DE LALANDE.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1807.

ARTICLE I.

To Mr. NICHOLSON,

SIR,

NOTWITHSTANDING the very ingenious inveſtigations of Dr. Wollaston and others, it appears to me, that the subject of looming, or horizontal refraction, is still capable of being explained with greater precision, and upon simpler principles: I shall therefore trouble you with a few observations, which have occurred to me respecting it. Looming, or horizontal refraction.

Let the refractive density of a medium be supposed to vary gradually and equally, in parallel strata; the variation beginning from a certain plane surface, and being continued, till, at a certain distance above that surface, the refractive power wholly vanishes. For example, the refractive density of air being expressed by 1.0003, if the temperature vary 1° in 1 foot, the refractive power will vary .000 000 6; and dividing 1.0003 by this, we have 1 666 667 feet, for the imaginary height of a medium continuing to vary at the same rate till its refractive power vanishes. Suppose strata regularly diminishing in refractive power.

Now upon the projectile hypothesis, supposing a particle of light to be initially at rest in this medium, it will be actuated by a constant accelerating force; and by falling from the top to the bottom, it will acquire the velocity natural to light in the original medium: and if a ray of light enter Progress of light through it.

the variable strata from the medium, its motion will be similar to that of a jet or a projectile rising in any direction from the bottom of a reservoir with the velocity due to its height.

Path of the ray of light.

Let A B (Pl.V. Fig. 1.) be the imaginary height; if we describe the semicircle A C B, a ray of light entering at B, in the direction B C, will describe the parabolic path B D E, B E being four times F C; or if the circle B G H be twice as great in diameter as A B, F D will be equal to G I. And, if several rays, passing from a point K, (Fig. 2.) enter the variable medium at the lower surface L M; making the semicircle K N equal to B G H, the distances K O, K P, will be equal to 2 Q L + 2 R S, and 2 Q T + 2 U X, respectively.

Now the distance K P must be a minimum, when the fluxions of Q T and U X are equal; that is, when K Y = X Z, (Fig. 3.) T Y being perpendicular to K T, and X Z parallel to K N. Make K α = K Q, and describe the semicircles K β, $\alpha\beta$, K β being half of K N; draw K γ perpendicular, and $\gamma\delta$ parallel to K N; then δ X, parallel to Q T, will determine the position of the point X so as to fulfil this condition. It is obvious that when K Q is very small in proportion to K N, δ X will coincide with Q T, and X will be in the intersection of the circle K N with the surface.

Apparent place of the object.

Consequently to an eye placed at K, (Fig. 4.) the object ϵ will be seen in the direction K X, and the object ζ in the directions K η and K ϑ; so that there will be two elevated images of the line $\epsilon\zeta$, the one erect, and the other inverted.

Double.

An inverted object only.

If the variable medium be only thick enough to admit the passage of the rays below K X, there will be no direct image, but an inverted one only. The inverted image will in general be nearly of the natural dimensions, although a little contracted; the case being nearly similar to a very oblique internal reflection. The points K and ϵ may be considered as conjugate foci, with respect to the refraction of the variable medium.

In the supposed case of the variation of a degree for each foot of air, K N being $3\frac{1}{2}$ million feet, if K Q be 1 inch, K X will be 527 feet, and K ϵ 700 yards. The angular deviation

deviation of the place of the point ε would be 32 seconds: the passage of the light through every 16 feet of the medium producing a total deviation of a second; and if the change of the air's density were more or less than $\frac{1}{500}$ in the space of a foot, the deviation would be as much more or less than a second in each space of 16 feet through which the light passes. The curvature of the earth's surface becomes a second in 102 feet; consequently a change of density amounting to $\frac{1}{3300}$ in a foot, or a change of temperature of a degree in 6 or 7 feet, would be sufficient to produce a refraction equivalent to the apparent depression of a distant object arising from this cause, and to elevate the coasts of a wide channel, so as to make them visible to each other. This result may also be more simply obtained from Simpson's investigations respecting atmospheric refraction, the refractive density being inversely proportional to the distance from the centre of the earth, when the temperature varies 1° in 6 or 7 feet; for, as Dr. Young observes in his extensive system of natural philosophy lately published, Vol. II. Art. 461, "If the refractive density of a medium vary as a given power of the distance from a certain central point, the angular deviation of a ray of light will be, to the angle described round the centre, as the exponent of the power to unity." Proportion of the deviation.

I am, Sir,

Your very obedient servant,

EMERITUS.

Postscript. If it be required to determine the position of X for the lowest ray that can cross the line ζ ε, supposing it to be at any other distance from K, we must make the rectangle N U X = ¼ N K ε; as may be understood by considering that the fluxion of the tangent of P K X is inversely as N U, and the fluxion of K U is as U X. This determination requires in general the solution of a biquadratic equation; but when K ε is very small in proportion to K N, U X will be very nearly ¼ K ε, or still more nearly ¼ K ε + K ε cub. ÷ 64 K N q. The point ε thus found will be the single point of the image as before: the length of the path of the ray within the variable medium will in both cases be Formula for determining the place of the image.

Formula.

half of the distance K ε; but the total deviation of the light will not be twice the angular displacement of the point ε, unless K and ε be equidistant from the surface. If however the angular direction of the surface be known, as is almost always the case in nature, the angle P K X, which is half the deviation, may also be found by observation; being, for example, when the surface is horizontal, the actual angular elevation of the image of the point ε. The place of the surface L M, which limits the variable medium, may be found from the measures of the actual elevation and the displacement of the point ε; for its distance from ε, the middle point between ε and its image, is always one fourth of the elevation. The circumstances will be nearly similar when K is either in the line L M, or a little above it, provided that ε be below it; but if both these points are above it, there will be no double image.

Variation of the medium continued to the eye and the object sometimes.

If however, the variation of the medium be continued, in an inferior degree only, to the place of the eye and the object, effects of a similar nature may still be sometimes produced: but it is not sufficient in this case to suppose with Dr. Wollaston, that the curve indicating the density has a contrary curvature; for it must be such, that the change of density, and consequently the curvature of the ray, must vary more rapidly than the distance from the line joining the eye and the object; for example, if the curve be logarithmic, its subtangent must be considerably less than the greatest distance of the bent ray from its chord; otherwise there can be no double image. Supposing the curvature of the rays be as the distance from any given line, the form will be nearly that of the harmonic curve. But whenever the object can be seen in its true place, beside the appearance of one or more displaced images, it is obvious that both the eye and the object must be situated in a uniform medium, as we have hitherto supposed.

Object seen in its true place.

II.

Fig. 1.

Theory of Looming or Horizontal Refraction.

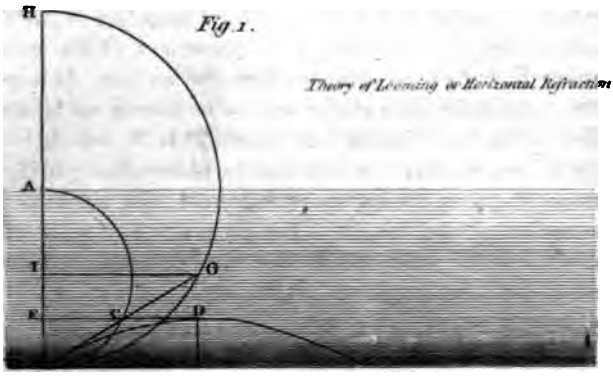


Fig. 2.

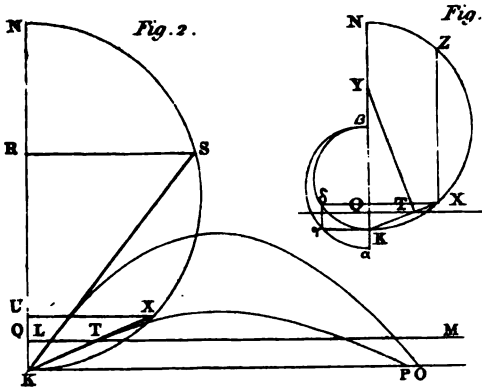


Fig. 3.

Fig. 4.

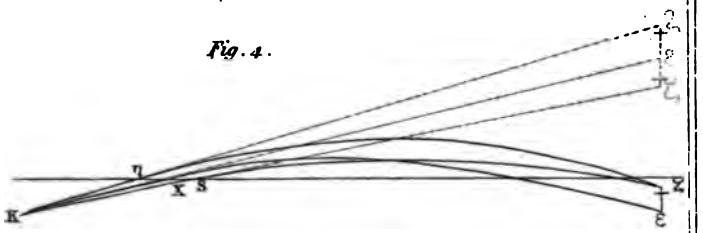




Fig. 1
Fig. 2
Fig. 3
Fig. 4
Fig. 5
Fig. 6
Fig. 7
Fig. 8
Fig. 9

II.

Remarks on Pruning Fir Trees; with an Engraving, to explain the Advantages of the Method recommended. By Mr.* ROBERT SALMON, *of Woburn, Bedfordshire.*

SIR,

I HAVE the honour of transmitting to you some observations on the management of fir plantations. Having had the care for some time past of such plantations, and knowing how much they are increased in this kingdom, I considered it as a matter of importance, that a proper mode of management should be generally known, in order to bring timber to the greatest attainable perfection. For this reason I have turned my thoughts to the subject, and am confident that much may be done, as is elsewhere asserted, by good management. I have collected several specimens to demonstrate the difference between good and bad management, and have made some observations, which I have not before met with, and may perhaps be useful; you will have the goodness, therefore, to present these observations and specimens to the Society of Arts, and to believe me,

Plantations of Firs much increased.

Your very humble servant,

Woburn, April 29, 1805. ROBERT SALMON.

To Dr. C. TAYLOR.

References to Plate VI. showing specimens of English grown Fir Timber, cut out of his Grace the Duke of Bedford's plantations at Woburn, pointing out the impropriety of leaving timber to the course of nature, and the loss and defects that arise from such mode of management; also illustrating the necessity of some fundamental rule for managing the same, and the advantage of early and close pruning off superfluous branches, with a general rule for

Necessity of early and close pruning.

* Transactions of the Society of Arts for 1806.

performing

performing the same, and regulating the distance of the plants in Fir Plantations.

Fig. 1. Section A. shows a dead knot and progress in the growth of the tree, having 19 years of growth below the bough, and 18 years above it. From the regular course of nature, as shown by this section, it is evident, this bough or knot must have existed as long as the upper part of the tree, namely, 18 years. For the first three years the growth and accumulation of the bough proceeded regularly with the tree; but about that time (now 15 years ago) the bough must have been distantly cut off, thereby preventing its regular increase in the part left remaining; for 6 years after cutting off it appears to have barely existed, and after that ceased to exist at all but as a dead bough. Since it became so, 9 years of accumulation have taken place on the trunk of the tree, thereby gradually enclosing a part of the dead bough, which part so enclosed is what by joiners is properly called a dead knot; the boughs that exist and are enclosed whilst living, are the live knots, and these the tree will produce either as the bough may be distant or close cut from the tree. From this specimen may be determined, that if the bough had been cut close to the tree at four years growth, there would now have been sound clean wood over it to the outside: or when it was cut, if it had been taken off at ⊕, sound clean wood would have formed over it to the outside.

igh left too
;.

N. B. In all the specimens this mark ⊕ is affixed to point out the proper place for cutting off, and is so placed as to allow for thickness of bark at the time it should have been cut.

Section B. shows a striking instance of the impropriety of leaving the smallest bough cut at a distance from the tree; this bough was cut off and became stagnant at 2 years growth, notwithstanding which it was 14 years before the wood on the trunk accumulated to the end of the dead knot.

ıer in-
aces of bad
ining.

Had this bough been cut at ⊕, the knot as far as that would have been firmly united with the tree, and above it all sound clean wood.

Fig. 2.

Fig. 2. Section C. Another instance of improper cutting. Bad pruning.

If it had been cut at ⊕, the timber would have been more valuable.

Section D. This before cutting exhibited a healthy bough, and the section shows it the same, exhibiting a live knot. This specimen clearly shows the progress of nature in healthy boughs; it also shows the great impropriety of suffering such boughs to exist more than 5 or 6 years, at which age had it been cut off, instead of a knot and great defect there would have been clean wood from ⊕ to the outside.

Fig. 3. Section E. Another striking proof of the impropriety of long cut boughs, a dead knot of many years standing, but far from being enclosed now, admitting wet into the heart of the tree: it should have been cut at ⊕.

Fig. 4. Part of another Scotch fir.

Section F. A very striking proof of young and bad pruning. This bough was cut at 4 years growth, and now, after 18 years accumulation of the trunk, remaining uncovered, and would so have remained many years longer.

Section G. A bough which, whilst standing, appeared not vigorous or healthy; the section shows that at 6 years it was in decline, and after that increased very little, though its increase may be distinctly traced to the present time.

This should years ago have been cut off.

Fig. 5. A very complete specimen of good pruning, though much too late. Instance of good pruning.

Section H. A large bough cut off at 6 years growth, but so close cut, that in 4 years afterwards the wood on the trunk of the tree is arrived at the extremity of the knot.

Fig. 6. and 7. Parts of a Weymouth Pine Fir, 31 years growth, a most striking specimen, and complete refutation to the doctrine of those who contend that the best way is to leave plantations to prune themselves. This tree grew near the outside of a thick plantation securely fenced, and in a state of nature at the time it was felled, except some accidental breaking off a few boughs near the bottom of the tree. Before it was felled it indicated sickness by the foliage, but from what cause it was so, no trace appeared, as the trunk bore a very healthy appearance. The section shows, by the small Wood left to itself.

small increase of wood for the last 6 years, that it was not then healthy.

Section I. A dead knot from a bough broke off at 6 years growth; since which 25 years growth of wood have formed on the trunk, without nearly covering the stump; this stump was broken off before representation.

Fig. 7. is a horizontal section of three other knots cut from the same round as figure 6, showing the great obstruction to the growth of the tree round the knots: this also explains the cause of the great hollow round the knot in fig. 6.

The knot at K is a most striking and undeniable proof of the impropriety of leaving the smallest bough for nature's disposal. This bough protruded beyond the tree some distance, and evidently never existed but in a stagnant state, for the last twenty-nine years; it being only two years old when it so became stagnant.

Fig. 8. A piece of the same tree as fig. 6 and 7; but from the next higher tier of boughs, having a small piece wasted between the two.

Section L. Two small boughs, the upper one only one year's growth, was twenty years before covered, and has nine years wood over it. The lower bough is of six years growth, was twenty years before covered, and has four years wood over it.

Perfection of Fir.

On contemplating these specimens; considering the purposes that fir timber is generally applied to, and having some knowledge of plantations of this sort, it must occur, that clearness of knots, straightness, length and equal size of its trunk, constitute its perfection; and, if deficient in all these, it is of no value but for the fire. Next to these considerations, and the prospect of an improved knowledge of cultivating this article, it may be a fair question, if our own country is not capable of producing fir timber little or not at all inferior to the foreign fir.

Fir may be produced here equal, if not superior to foreign.

At present firs in this country appear not for any period to have been considered much otherways than as ornamental. For this purpose they serve but for a certain time, which past, it has been their fate to be cut down long before having attained maturity. But from the vast plantations now established, it is to be hoped, that another century may obtain

to

to English Fir some of the character of the English Oak; towards such end, if attainable, every means should be used, and towards it nothing appears more likely to succeed, than a well grounded general practical mode of management, from the time of their being planted out, to their greatest imaginable age of improvement. That a knowledge of such may by perseverance be gained, is not much to be doubted; and by inspecting and considering the specimens herein referred to, there appears great reason to conclude, that early and proper pruning and thinning will form a considerable feature in the system to be adopted.

Now as forms are first instruments in good systems, and as proceedings on fundamental principles (though in the essay they may a little err) are better, in a general view, than occasional success by hazard; so it may be warrantable that a system for general management may be laid down, although the author cannot possibly have lived to prove all by experience: so the rules hereafter submitted are given, being the result of only a few years observations.

Planting thick

For planting, from every authority or observation, there can be no doubt that all firs should be planted thick; not more than four or five feet apart.

Not several species together.

Where firs of the same kind are planted together, there is less loss of plants from one sort overgrowing and destroying the others; consequently it appears adviseable, that all the different sorts be planted by themselves. If any admixture be at all admitted, the Scotch and larch may best succeed: but this is not certain, and they will certainly be best separate on two accounts; first, because they are not so likely to injure each other; and secondly, the larch may be put into the ground best suited to them, and the Scotch the same.

Spruce as a skreen.

In making plantations of any particular sort, it may be right to have a few spruce, or other sorts on the outside, to prevent mischief from sudden gusts of wind; but if the situation is not subject to such gusts, the spruce had better be omitted, being mechanical agents only, and by excluding the sun and air they act against the operation of nature.

Ornamental plantations.

In these hints ornament is not considered; if such be wanted, and profit also, then the spruce, larch, silver, and some others may be combined.

From

Rules for pruning.

From some years observations on pruning and the effects thereof, it appears certain, that Fir trees, at a certain age, should be pruned to a certain height; and for regulating thereof, the following simple rule is recommended. The pruning to commence when the trees are six years old, or when there is discernible five tiers of boughs and the shoot; the three lower tiers of boughs are then to be taken off. After this first pruning, the trees to be let alone for four or five years, and then, and at every succeeding four or five years, the pruning to be repeated, till the stem of the tree is clear to forty feet high, after which, as to pruning, it may be left to nature. The rule for the height of pruning, after the first time, to be half the extreme height of the tree, till they attain twenty years growth; and after that time, half the height of the tree, and as many feet more as it is inches in diameter at four feet from the ground. This pruning is known from repeated observations not to be excessive; and the rule is calculated to check the too tapering top, and strengthen the slender bottom, by carrying the pruning to a greater proportionate degree, in a ratio compounded of the height and bottom bulk; and by this rule it may be observed, that the trees will be at top clothed with somewhat less than half their branches. The proper time for pruning is between September and April, and the tool to be used, the saw.

Thinning.

Orderly thinning the trees at certain periods is the next essential to pruning, and for this purpose observations have been made on the most orderly and thriving plantations, and the following simple rule is recommended. Keep the distance of the trees from each other equal to one fifth of their height. In the application of this rule for thinning, it is evident, that each individual tree can never be made to comply; for the original distance (even if planted in the most regular order) will allow only of certain modifications, by taking out every other tree, and so on; but even if the obtaining such equal distance was practicable, experience would show that another way should be preferred, of which the eye must be the judge, by taking out such trees as are least thriving, stand nearest another good tree, &c.; at the same time keeping in view the rules prescribed: the following

ing of which rules may easily be proved by measuring a chain square, or any quantity of the land, and counting the trees thereon; then by trying the height of two or three trees in that quarter, and taking one fifth of such for the distance, it would be readily seen how many trees should be contained in the piece measured: or the practice may more simply be regulated, by taking the distance of eight or ten trees added together, the average of which should be equal to a fifth of the height of the trees.

In these rules nothing impracticable or complicated is proposed.

Produce of trimming more than pays expense.

The author has for years known the expense and produce from trimming only, and finds in Bedfordshire the produce doubly repays the expense; and although some experimentalists may differ from him, or time may show some reason for deviating somewhat from his rule, yet it is presumed all will agree that some simple system is adviseable, instead of having plantations and woods mismanaged, to the great loss of the community and the proprietors. If such a system as proposed be generally promulgated; if not perfect, it will most likely, in time, become so, and thereby have its advantage; and that some advantage may be had in speculation, the following concluding remarks are introduced.

Pruning garden trees.

In the common course of gardening, it is understood, that pruning invigorates the tree; that trimming off the side branches makes the upright ones shoot the stronger, and by cutting out the dead and decayed wood the tree is kept alive: some of this doctrine will certainly apply to the tribe of firs; it will certainly substitute clean wood for knots, and of all this treatment, from their particular uses, they of all other trees stand in most need, and will be most improved by it. And should it be admitted, that like treatment would on the fir, as well as other trees, produce the like effect, it would lead to a well-grounded expectation, that, as well as producing clearness from knots, straightness, and length, the same operation would advance the quality nearer to that of foreign fir; for it may be traced, that where trees are tall and clear of boughs or knots, the whole substance of the wood is better and of finer grain, and it appears likely, that such will always be the case: the reason may probably be inferred

Advantage of pruning Firs.

inferred from the sap having farther to rise and descend, and having no boughs to divert or delay it, the circulation must be more fine and rapid, most increase be left in the neighbourhood of the boughs at the top of the tree, and least on the sides at the lower part; consequently adding to the length of the head, and rendering more fine each annual increase to the body; thereby producing a close-grained, clean, long, and regular, easy-tapering, useful piece of timber; instead of a coarse-grained, short, sudden-tapering trunk, with a quantity of boughs and knots.

Applicable to other timber.

The foregoing observations and rules are meant to apply to fir timber only, but to a certain degree they may be applied to other timber; though by no means to the same extent, or age. But if applied as far as the first fourteen years of their growth, and then the pruning altogether omitted, and the thinning-out very much increased, any plantation would be rendered much more valuable, than if left entirely to nature.

ROBERT SALMON, Surveyor.

Woburn, April 2, 1806.

III.

Abstract of a Memoir read at the Meeting of the fifth Class of the Institute, September the 29th, 1806, by Mr. LAPLACE, *on the apparent Attraction and Repulsion of small Bodies floating on the Surface of Liquids**.

Bodies floating on fluids attracted toward each other,

when they are of the same matter.

IN the theory I have given of capillary attraction, I have subjected to analysis the attraction of two vertical and parallel planes, very near each other, with their lower extremities immersed in a fluid. I have shown, that, if they be of the same matter, this action tends to bring them nearer together; whether the planes elevate the fluid near them, as ivory immersed in water; or depress it, as laminar talc, on which we feel a kind of unctuosity, that prevents them

* Journal de Physique, Vol. LXIII. p. 248.

from

from being wetted. Each plane is then pressed toward the other plane by a force equal to the weight of a parallelopipedon of the fluid, the height of which is half the sum of the elevations above the level, or depressions below it, of the extreme points of contact of the interior and exterior surfaces of the fluid with the plane; and the base of which is that part of the plane included between the two horizontal lines drawn through those points. This theorem includes the true cause of the apparent attraction of bodies swimming on a fluid, when it is elevated or depressed around them. But experience shows, that bodies repel each other, when the fluid rises toward one of them, and is depressed toward the other. Having applied my analysis to these repulsions, it has led me to the following results, which I conceive may be deemed interesting by the natural philosopher and geometrician, and complete the theory of capillary attraction.

Force of this attraction.

But they are repelled, if one elevate the fluid, the other depress it.

If we still suppose the bodies to be vertical and parallel planes, the section of the surface of the fluid included between them by another vertical plane perpendicular to these will have a point of inflexion, when the two planes are a few centimetres [a centimetre is near four lines English] from each other. If they be brought nearer together, the point of inflexion will approach nearer to that plane, toward which the fluid is depressed; if the depression of the fluid in contact with the exterior side of that plane be less than the elevation of the fluid in contact with the exterior side of the other plane. If the contrary be the case, the point of inflexion will approach the other plane. This point is always in the level of the fluid in the vessel, in which the planes are immersed. The elevation and depression of the fluid in contact with these planes are less at the interior surface than at the exterior. In this state the planes repel each other. On continuing to bring them nearer, the repulsion still subsists, as long as there is a point of inflexion. This point at length coincides with one of the planes. The repulsion still continues beyond this period; but on continuing to bring the planes nearer together, the repulsion becomes null, and is converted into attraction. At this instant the fluid is equally elevated on each side of the plane that is capable of being wetted; and it is as much elevated above the level at the interior

Theory.

Circumstances under which the repulsion takes place.

At length annihilated, and becomes attraction, when the fluid is equally elevated on each side of one plane, and as much depressed on one side of the other.

interior of the other plane, as it is depressed below it at the exterior. Thus the repulsion is changed into attraction at the same moment in each plane. On bringing them still nearer, they attract each other, and proceed to unite with an accelerated motion. These planes therefore exhibit the remarkable phenomenon of an attraction at very small distances, that is changed into repulsion beyond certain limits; a phenomenon which nature presents likewise in the inflexion of light near the surfaces of bodies, and in the attractions of electricity and magnetism. There is one case, however, in which the planes repel each other, however small their distance may be; and this is where the fluid is depressed near one of them as much as it is raised near the other. Here the surface of the fluid has constantly an inflexion in the middle of the interval between them.

Case of constant repulsion

The integration of the differential equation of this surface in general depends on the rectification of conic sections, and consequently it is impossible to obtain it in finite terms. But it becomes possible, when the planes are at the distance where repulsion is changed into attraction; as this distance can then be determined in a function of the elevation and depression of the fluid at the exterior of the planes. Thus we find, that it is infinite, if the depression of the fluid on the exterior of the plane incapable of being wetted, be infinitely small: whence it follows, that the two planes never repel each other then. This may take place too even in the case where the fluid is perceptibly depressed at the exterior of the latter plane: as it is sufficient, if friction keep the fluid a little more elevated at the interior of the plane, than it would be if this friction did not exist; an effect analogous to that daily perceived in the barometer, when the quicksilver is falling. We find too by this analysis, that, if the surface of the plane capable of being wetted come to be made wet, the two planes will begin to attract each other at a very perceptible distance, greater than that at which they began to attract each other before. It is not the truth therefore to say, that two planes, one capable of being wetted, the other not, always repel each other. The same thing happens here as with two balls having the same kind of electricity, as these attract each other notwithstanding, when we vary in a suitable

The equation of the curve of the surface not obtainable in finite terms,

except within a certain distance.

Modified by friction.

When the surface is wetted it attracts at a greater distance.

ble manner the respective intensities of their electricity,
their distance.

y means of the two following theorems, we may calcu- Calculation of the attraction or repulsion.
the tendency of the planes toward each other, or their
ual repulsion.

Whatever be the substances of which the planes are form- Theorem I.
he tendency of each of them toward the other, is equal
ie weight of a parallelopipedon of the fluid, the height
hich is the elevation of the extreme points of contact of
fluid with the interior plane, minus the exterior eleva-
; the depth half the sum of these elevations; and the
dth that of the plane in a horizontal direction. We
t consider the elevation as a negative quantity, when it
anged into depression below the level. If the product
he three preceding dimensions prove negative, the ten-
y become repulsive.

When the planes are very near together, the elevation of Theorem II.
fluid between them is the inverse ratio of their mutual
ance; and is equal to half the sum of the elevations, that
ld have taken place, if we suppose the first plane to be
he same substance as the second, and then the second
ie to be of the same substance as the first. We must ob-
e too, that the elevation must be put as negative, when
anges into depression.

We see by these theorems, that in general the repulsive The repulsion weaker than the attraction.
e is much weaker than the attractive, which displays itself
n the planes are brought very close together, and must
l carry them toward each other with an accelerated mo-
. In this case the elevation of the fluid between the
es is very great, relatively to its elevation near the same
es exteriorly. If therefore we neglect the square of the
er elevation, with respect to the square of the former,
fluid parallelopipedon, the weight of which expresses the
dency of one of the planes toward the other, in virtue of
first of the preceding theorems, will be equal to the
duct of the square of the elevation of the interior fluid,
half the breadth of the plane in the horizontal direction.
is elevation being, by the second theorem, reciprocal to
mutual distance of the planes; the parallelopipedon will
be

be proportional to the horizontal breadth of the plane divided by the square of this distance. The tendency of the two planes toward each other, therefore, will follow the general law of attraction, that is to say, it will be in the inverse ratio of the square of the distance.

Follows the general law of attraction.

Put to the test of experiment,

Desirous of knowing how far these results of my theory were agreeable to nature, I requested Mr. Haüy to make some experiments on this delicate and curious point in natural philosophy. He complied with my wishes, and found the formulæ in perfect unison with experience. He particularly ascertained the singular phenomenon of an attraction changed to repulsion by the increase of distance, as the following note I received from him will show.

and found to be agreeable to it.

Abbé Haüy's experiments on the subject.

"I suspended a small square leaf of laminar talc to a very slender thread, in such a manner, that its lower part was immersed in water. In the same water, at the distance of a few centimetres, I immersed the lower part of an ivory parallelopipedon, so that one of its faces was parallel to the leaf of talc. I then caused the parallelopipedon to advance very slowly toward the leaf of talc, keeping it still in a parallel position, and stopping at intervals, to be certain the motion, that might be imparted to the fluid, did not sensibly affect the experiment. The leaf of talc then receded from the parallelopipedon; and when, on continuing to move the latter with extreme slowness, there remained but a very small distance between the two bodies, the leaf of talc suddenly approached the parallelopipedon, and came into contact with it. I then separated the two bodies, and found the parallelopipedon wetted to a certain height above the level of the water; and on repeating the experiment, without wiping it, the attraction began sooner; sometimes indeed it took place from the first, without being preceded by any perceptible repulsion. These experiments, carefully repeated several times, always afforded the same results."

IV.

IV.

ct of a Memoir on the Adhesion of Bodies to the Sur- of Fluids, read at the Sitting of the first Class of the itute, November the 24th, 1806. By Mr. LAPLACE*.

REAT number of experiments have been made on hesion of bodies to the surface of fluids, but without spicion, that this adhesion was the effect of capillary . Dr. T. Young appears to me to be the first, who this ingenious remark †. On applying my analysis to periments, I have found, that it represents them as could be expected with regard to experiments so very e, and not always agreeing exactly with each other. henomena of capillary action being now reduced to a matical theory; nothing more is wanting to this inte- branch of natural philosophy, but a series of accu- periments, in which every thing capable of affecting ult of this action is carefully removed. The want of recise experiments is felt, in proportion as the sciences e toward perfection. To the concurrence of the great eries in mechanics and mathematics with those of the pe and pendulum, astronomy is indebted for its vast s. We cannot therefore too strongly invite the phi- er to give the greatest precision to his operations; as not sufficiently encourage the skilful artist, who de- is labours to the improvement of the instruments of e. A single experiment badly executed, has frequently he cause of many mistakes; while an experiment well med subsists for ever, and sometimes becomes a source covery. On such an experiment we rely with confi- ; but the cautious inquirer feels himself under the ity of verifying the results given by an observer, who t acquired a solid reputation for accuracy.

Dr. Young first accounted for the adhesion of bodies to the surfaces of fluids by capillary attraction.

Theory of capillary attraction wants only experiments.

Cause of the progress of astronomy.

Necessity of accuracy in experiments.

urnal de Physique, Vol. LXIII. p. 413. Nov. 1806.

ilosophical Transactions for 1805: or Journal, Vol. XIV. p. 74,

Glass on the surface of water resists separation with a force proportional to its size.

When a disk of glass is applied to the surface of water standing at rest in a vessel of considerable extent, on endeavouring to separate it from the water we find a resistance proportional to the surface of the glass. On raising the glass, we raise at the same time a column of water above

Cause of this.

the level of the surface, which resembles in its figure the grooved wheel of a pulley. Its base extends indefinitely on the surface of the level: as the column proceeds upward it diminishes to about seven tenths of its height: above this it enlarges, till its summit covers the surface of the disk. To determine its volume, let us conceive in the plane of its least diameter an interior canal, at first horizontal, afterward curved vertically as far as the level surface of the fluid, and at that point resuming its horizontal direction. It is easy to perceive, that, in the case of the column being in equilibrium, the power owing to the capillariness of its surface must balance the weight of the fluid in the vertical branch of the canal. On raising the disk higher, the weight becomes more powerful from the capillary attraction, and the column separates from the disk. The weight of the column of water raised in this state of equilibrium is the measure therefore of the resistance experienced in separating the disk. If the breadth of the disk be considerable, we find by analysis, that this weight is equal to that of a cylinder of water, the base of which is equal to that of the disk, and the height the product of one millimetre [0·391 of a line] multiplied by the square root of the number of millimetres in the height to which water would rise in a tube of glass one millimetre in diameter. The surface of the water is a tangent to that of the disk; but if these two surfaces cut each other, the preceding result must be multiplied by the cosine of half the acute angle formed between them, and divided by the square root of the cosine of the entire angle.

Case where the fluid would sink in a capillary tube.

When the fluid, instead of rising, would be depressed in a capillary tube of the same materials as the disk, as mercury is in a tube of glass, the column raised by the disk has no longer the shape of a pulley: its base extends indefinitely on the surface of the fluid, but the column decreases continually from this base, till it comes into contact with the disk.

The weight of this column, in a state of equili-
m, is equal to that of a fluid cylinder, the base of which
e surface of the disk, and the altitude the product of
millimetre, multiplied by the number of millimetres the
would sink in a tube of the same material as the disk,
of one millimetre in diameter, this product being mul-
d by the line of half the acute angle that the surface
e fluid forms with the disk, and divided by the square
of the cosine of the whole angle.

ll these results require a slight correction, relative to the
osition of a great diameter of the disk. I shall give
correction, which may be neglected without any sensi-
error for disks the diameter of which is thirty millime-
[11·74 lines] and upward.

The theory compared with experiment.

o compare the preceding results with experiment, let us
ider a disk of glass 100 millimetres [3 in. 9 l.] in diame-
Mr. Haüy has observed, that in a tube of glass one
metre in diameter, water would rise 13.569 millimetres
e the level: whence it is easy to conclude, by means of
theorem above given, that the force necessary to sepa-
the disk from the surface of water would be equal to a
ht of 28·931 grammes [446·819 grs.]. Now according
Ir. Achard this force is 29·319 grammes [452·832 grs.],
h differs very little from the preceding result. I made
experiments on the resistance opposed by a disk of
applied to the surface of mercury. But to compare
with the theory, it is necessary to know the angle
ed by the surface of this fluid in contact with the glass.
experiment of this kind, made with precision, is well
ted to determine this angle, which appears to be of 30°
0°.

Two disks of glass with a stratum of water between them.

we place two disks of glass horizontally on each other,
ng between them a very thin stratum of water, these
disks will adhere with considerable force. To determine
force, it must be observed, that the interposed fluid
takes the form of a pulley; and that the smallest ra-
of curvature of its surface is very nearly equal to half
thickness of the stratum. Neglecting here then, as may
one when the disks are very large, the greatest radius of
ature, we find the resistance, that the two cylinders op-

pose to their separation, equal to the weight of a cylinder of water, the base of which is the surface of the disk, and its altitude the height to which water would rise between two parallel planes of glass, as distant from each other as the interval that separates the disks. Mr. Guyton de Morveau made an experiment of this kind with two disks of glass, the diameter of which was 81·21 millimetres [3·18 inches], and he found their resistance to separation 250·6 grammes [3870·5 grs.]. According to the preceding theorem, the resistance would be only 155·78 grammes [2406 grs.]. The difference of about one third between these two results, arose, no doubt, either from the estimation of the interval that separates the disks, which requires great nicety in such small intervals; or to the inequalities of the surfaces of the disks, which it is difficult to render accurately plane.

Guyton found the resistance much greater than it should have been by theory,

from mistaking their distance,

or inequalities of their surface.

Theory of small bodies sustained on fluids.

The sustentation of small bodies on the surface of fluids depends on this general principle: "The diminution of weight of a body merging in a fluid, that sinks around it by capillary action, is the weight of a volume of fluid equal to that of the part of the body beneath the level, added to the weight of the volume of fluid displaced by capillary action. If this action raise the fluid above the level, the diminution of weight of the body is the weight of a volume of the fluid equal to the part of the body below the level, *minus* the weight of the fluid raised by capillary attraction."

Diminution of weight of bodies in fluids.

This principle embraces the known hydrostatical principle of the diminution of weight of a body plunging into a fluid: it is sufficient to omit what relates to capillary action, which totally disappears, when the body is completely in the fluid below the level of its surface.

Demonstration of this principle.

To demonstrate the principle just laid down, let us suppose a vertical tube large enough to include the body itself, and all the body of fluid that it sensibly raises, or the space it leaves empty by capillary action. Let us conceive this tube, after having penetrated into the fluid, to bend horizontally, and afterward rise vertically, preserving the same diameter throughout its whole extent. It is clear, that, in the case of an equilibrium, the weights in the two vertical branches of the tube must be equal. The weight of the body

body therefore must compensate the vacuum it produces by capillary action; or, if it raise the fluid by this action, its inferior specific gravity must compensate the weight of the fluid raised. In the first case this action raises the body, which by this means may be retained on the surface, though specifically heavier than the fluid: in the second case it has a tendency to sink the body in the fluid. It is thus that a very slender cylinder of steel, the contact of which with water is prevented either by a varnish, or a thin stratum of air surrounding it, is supported on the surface of the fluid.

Steel floating on water.

If we place thus two equal and parallel cylinders, touching each other, but the extremity of one passing beyond that of the other, we perceive them immediately sliding by each other to bring their extremities on a level. The reason of this phenomenon is visible. The fluid is more depressed by the capillary action of the two cylinders at that extremity of each which is in contact with the other cylinder, than at the opposite extremity. The base of the latter extremity therefore experiences greater pressure than the other base, since the fluid around it is more elevated. Consequently each cylinder tends to unite with the other more and more: and as the accelerating forces always carry a system of bodies, the equilibrium of which is deranged, beyond the state of equilibration; the two cylinders must alternately pass each other, producing an oscillation, which, diminishing incessantly, by the resistance the cylinders experience, will at length be annihilated. The cylinders, being thus arrived at a state of rest, will have their extremities parallel. These oscillations may be determined by analysis, and we may compare the theory of capillary action on this point with experiment.

Two equal pieces of thin wire, partly in contact, will become so throughout their whole length.

These comparisons are the true touchstone of theories, which leave nothing to be wished, when by means of them we can not only foresee all the effects that must result from given circumstances, but determine their quantities with accuracy.

Theory should be tested by experiment.

If we consider the whole of the phenomena of capillary action, and their dependance on one single principle of an attraction between the molecules of bodies decreasing in a very rapid ratio, it is impossible to call this principle in question. This attraction is the cause of chemical affinities: it

Capillary attraction depends on that of the molecules of bodies, which is the cause of chemical affinities.

it does not stop at the surface of bodies, but, penetrating into them to depths which, though imperceptible to our senses, are very sensible in the action of affinities, it produces that influence of masses, the effects of which have been displayed by Mr. Berthollet in such a happy and novel manner. Combined with the figure of capillary spaces, it gives rise to an almost infinite variety of phenomena, which, like those of the celestial bodies, are now brought within the domains of analysis. Their theory is the most intimate point of contact between chemistry and natural philosophy; two sciences, which now approach each other on so many sides, that one cannot be cultivated with much success, without a thorough knowledge of the other.

and accounts for the influence of masses on them.

Allies chemistry with physics.

Capillary attraction produces a catenary curve,

The resemblance of the figure of fluids raised, depressed, or rounded by capillary action, with the surfaces generated by the curves known under the name of catenary, linear, and elastic, on which mathematicians employed themselves at the origin of the infinitesimal calculus, led some philosophers to suppose, that the surfaces of fluids had a uniform tension, like elastic surfaces. Segner, who appears to have been the first that suggested this idea*, was well aware, that it could be no more than a fiction, adapted to represent the effects of an attraction between the molecules decreasing with great rapidity. This able mathematician endeavoured to demonstrate, that this attraction must have the same results: but, if we examine his reasoning, it is easy to perceive its inaccuracy; and we may conclude from the note appended to his researches, that he seems not to have been satisfied with it himself. Other philosophers, resuming the idea of a uniform tension of fluid surfaces, have applied it to various capillary phenomena. But they have not been more successful than Segner, in the explanation of this force; and the most able have contented themselves with considering it as a means of representing the phenomena. In giving into all the conjectures, which may arise from the first view of these phenomena, we may hit on some truths; but they will almost always be mingled with many errours, and the discovery of them belongs only to him, who, separating

but the surfaces of fluids have not a uniform tension.

Conjecture may hit upon truths,

but he is the discoverer who observes or in-

* Mem. of the Royal Society of Gottingen, Vol. I.

them

them from this mixture, goes so far as to establish them on solid foundations by observation or mathematical investigation.

vestigates them with accuracy.

V.

Account of a Loom to be worked by Steam or Water; by Mr.* JOHN AUSTIN, *of Glasgow.*

SIR,

AFTER much trouble, expense, and reiterated experiments, I have happily succeeded in completing a new WEAVING-LOOM, a Working-Model of which, with cloth in it, is presented to the Society for their inspection. It has, upon trial, succeeded beyond expectation, answers in every respect the purpose for which it is intended, and has met with the approbation of manufacturers of the first respectability in the country.

New looom for weaving.

After many different attempts, I think that I have brought my weaving-loom, which may be driven by water, or steam, to such a state of perfection, as to prove its utility, the more it is known and employed.

Worked by water or steam

My first attempt was made in the year 1789: I at that time entered a caveat for a patent, but relinquished the idea of obtaining one, and have since made many improvements upon my original plan. In 1796, a report in its favour was made by the Chamber of Commerce and Manufactures at Glasgow; and in the year 1798, a loom was actually set at work, at Mr. J. Monteith's spinning-works, at Pollockshaws, four miles from Glasgow, which answered the purpose so well, that a building was erected by Mr. Monteith, for containing thirty looms, and afterwards another to hold about two hundred.

attempted long ago, and carried into execution.

The model now submitted for inspection is an improvement upon those constructed for Mr. Monteith.

* Transactions of the Society of Arts, 1806. The gold medal of the Society was voted to Mr. AUSTIN for this invention.

The

Enumeration of its advantages.

The following are the advantages which my Loom possesses.

1. That from 300 to 400 of these looms may be worked by one water-wheel, or steam-engine, all of which will weave cloth, superior to what is done in the common way.

2. That they will go at the rate of sixty shoots in a minute, or two yards of a nine hundred web in an hour.

3. That they will keep regular time in working, stop and begin again, as quick as a stop watch.

4. They will keep constantly going, except at the time of shifting two shuttles, when the weft on the pirns is done.

5. In general, no knots need to be tied, and never more than one, in place of two, which are requisite, in the common way, when a thread breaks.

6. In case the shuttle stops in the shed, the lay will not come forward, and the loom will instantly stop working.

7. They will weave proportionally slower, or quicker, according to the breadth and quality of the web, which may be the broadest now made.

8. They may be mounted with a harness, or spot heddles, to weave any pattern, twilled, striped, &c.

9. There is but one close shed, the same in both breadths, and the strain of the working has no effect on the yarn behind the rods.

10. The bore and temples always keep the same proper distance.

11. There is no time lost in looming, or cutting out the cloth; but it is done while the loom is working, after the first time.

12. The weft is well-stretched, and exactly even to the fabric required.

13. Every piece of cloth is measured to a straw's breadth, and marked where to be cut, at any given length.

14. The loom will work backwards, in case of any accident, or of one or more shoots missing.

15. Every thread is as regular on the yarn beam as in the cloth, having no more than two threads in the runner.

16. If a thread should appear too coarse or fine in the web, it can be changed, or any stripe altered at pleasure.

17.

17. They will weave the finest yarn, more tenderly, and regularly, than any weaver can do with his hands and feet.

18. When a thread either of warp or weft breaks in it, the loom will instantly stop, without stopping any other loom, and will give warning by the ringing of a bell.

19. A loom of this kind occupies only the same space as a common loom; the expense of it will be about half more; but this additional expense is more than compensated by the various additional machinery, employed for preparing the yarn for the common loom, and which my loom renders entirely unnecessary.

20. The reeling, winding, warping, beaming, looming, combing, dressing, fanning, greasing, drawing bores, shifting heddles, rods, and temples, which is nearly one half of the weaver's work, together with the general waste accompanying them, which is about six per cent of the value of the yarn, and all which occur in the operations of the common loom, do not happen with my loom, which, by its single motion, without further trouble, performs every operation after the spinning, till the making of the cloth is accomplished; by which, independent of the saving of the waste, the expense incurred for reeling, warping, winding, &c. is saved, amounting to above twenty per cent of the yarn.

21. The heddles, reed, and brushes, will wear longer than usual, from the regularity of their motion.

22. More than one half of workmanship will be saved; one weaver and a boy being quite sufficient to manage five looms of coarse work, and three or four in fine work.

These advantages, which from experience my weaving-loom has been found to possess, and which upon inspection will be perceived, will, I presume, be esteemed of some magnitude.

My loom, as now constructed and improved, is much simplified, so that the manual labour requisite is trifling; and if it is encouraged by the Society of Arts, I am sensible much advantage will arise from their approbation, and the publicity it will in consequence receive.

I am, Sir,
Your humble servant,
JOHN AUSTIN.

Certificates

Certificates of its utility.

Certificates were produced from Messrs. HUGH CROSS, MATTHEW PERSTEN, and DAVID MUTRIE, dated Glasgow, October 12, 1796, stating that, by appointment of the Chamber of Commerce in Glasgow, they had inspected the Loom, constructed by Mr. Austin, and were of opinion, that it will be found to contain some ingenious and useful improvements, by producing saving and facility in several of the ordinary operations.

Messrs. NEIL, MACVICAR, and THOMAS HENDERSON, of Edinburgh, certified on the 12th of April, 1804, that they had seen, in the Trustees office there, the model of Mr. Austin's Loom, and that they thought it ingenious, and the best they had then seen.

Further Certificate, from Edinburgh, dated April 14, 1804, from Mr. JOHN DRUMMOND, and from Messrs. JAMES REID and JOHN WAUGH, partners in the house of WALTER BIGGER and Co. linen-manufacturers, testify to the ingenuity of Mr. Austin's Loom, and that it is capable of being employed to the great advantage of the manufactures of this country.

Mr. AUSTIN having left a complete Working-Model of his Loom with the Society of Arts, &c. a reference to it will convey an idea of its principles, better than any description that might be attempted; as from the variety of minute parts in it, the Committee of the Society have thought it impossible to have a drawing of it, upon their usual scale, which can be rendered sufficiently intelligible.

VI.

Observations and Experiments respecting the Art of making Copies of Written Paper by Pressure. *By* R. T.

SIR,

Copying machine.

A Few years ago, a Machine, called a Copying Machine, was offered to the Public for the purpose of obtaining a copy from any recently written paper.

To merchants and others, who are in the habit of writing a great number of letters, &c. of which they wish to have a copy

copy, this invention has been of so great utility, that it has now come into very general use.

The method of using this instrument, which is a rolling press, is briefly this: Having covered the paper to be copied with a piece of damp copying-paper (a kind of white, thin, unsized paper, made on purpose), place it between oiled papers on a board, cover it with some blotting paper, and pass it through the press: a copy, which is legible through the copying paper, is thus obtained from writing that has been written only a few hours. Thus far this machine fully answers the purpose; but when old writing is made to undergo this process, no effect is produced. Method of using it. Does not answer for old writing.

Should any method be discovered of obtaining copies from old writing, it would prove a valuable acquisition to many persons, and to me in particular. It is with the view of obtaining information on this subject, that I have troubled you with this letter; you, or some of your correspondents will, I hope, through the medium of your valuable Journal, favour the public with some communication, that may throw a light on this interesting subject. Information on this head desired.

The following account of a few experiments I have made will, perhaps, be of service in forming a judgment as to the means most likely to succeed; or they may be useful to any one, who may choose to prosecute the matter experimentally.

I remain, Sir,

Your obedient servant,

To Mr. NICHOLSON. R. T.

I first tried the most violent pressure (both with and without the substances hereafter mentioned), without advantage. A moderate pressure is best. Writing can seldom be got out after it has been written more than 24 hours. Experiments. Moderate pressure best.

In taking off writing, a considerable improvement was discovered; it consists in covering the copying paper with flannel instead of oiled paper. By glueing the copying paper on a piece of white paper, the writing is rendered more legible. Notwithstanding these, and using boiling water instead of cold, the old writing continued refractory.

Mechanical

Solutions applied to the writing, or the copying paper

Mechanical means having failed, it was necessary to encrease the power by the assistance of chemistry.

With this intent, I soaked either the old writing or the copying paper in various solutions, and passed them through the press. Old writing is rendered blacker by being soaked for some hours in infusion of galls, but it has no power to bring it on to the copying paper.

Infusion of galls.

Green vitriol.

Solution of sulphat of iron produced no effect.

Prusiate of potash.

With triple prussiate of potash a faint copy was sometimes obtained; often it had no effect: a few drops of sulphuric acid added to it increased its power, but the whole was rendered green.

Hidrosulphuret of ammonia has more powerful action on writing than any thing I have yet tried. When this liquid is poured on faded or almost any kind of writing, it changes it to an intense black colour. By the aid of this preparation, I have been enabled sometimes to procure tolerably good copies, but could not obtain a constant effect, though I often varied the progress.

Hidrosulphuret of ammonia.

But the action of hidrosulphuret of ammonia is incomplete, the black it gives to writing is not permanent, and on some writing it has no effect.

As this substance appeared more likely to succeed than any other, I was induced to examine it more particularly, but the result has convinced me its power is inadequate to the purpose.

The reason it gives a black colour to writing is this: almost all inks contain an excess of sulphate of iron; the ammonia combines with the acid, and the sulphuretted hidrogen with the iron, forming the black colour; and because different inks contain different proportions of sulphate of iron, they will not be equally affected by the hidrosulphuret of ammonia.

Hidrosulphuret of iron decomposed by the air.

The hidrosulphuret of iron is decomposed by the carbonic acid of the atmosphere.

The following experiments will prove these positions. Write on paper with;

No. 1. A solution of tan.

No. 2. A solution of green sulphate of iron.

No.

No. 3. A pale ink formed with green* sulphate of iron and solution of tan, having excess of sulphate.

No. 4. An ink as above with excess of tan.

Pour on the writings hidrosulphuret of ammonia; Nos. 1 and 4 will remain unchanged, Nos. 2 and 3 will instantly become intensely black, but No. 2 changes by exposure to a rusty brown, and No. 3 becomes faint.

If hidrosulphuret of ammonia is poured into solution of sulphate of iron, a black powder precipitates; when this is exposed to the air it turns to a red rust.

Although the hidrosulphuret appears to have no effect on dry tannate of iron, yet when poured into the ink No. 4, it changes it to a red colour; writing written with this mixture becomes nearly black in an hour. When filtered, a red substance remains, and the filtered liquor is of the same colour.

I have somewhere seen it asserted, that ink consists of a black powder suspended in water, so extremely fine as to pass with the liquor through a paper filter; this is not exactly the case.

Ink not simply a black powder suspended in water.

If ink, prepared as No. 3, be exposed to the air a short time and filtered, a black mass remains on the filter, and the liquor that passes through is of a fine deep blue colour: if a drop be let fall from the filter on a piece of ivory, and examined immediately, it will appear a homogeneous liquor, but in the course of a minute numerous black particles will be seen floating in it.

These effects are best perceived with a glass. Some of the filtered ink placed in a wine glass is speedily covered with a film; on shaking the glass, black pieces will be seen in the apparently colourless liquid, that trickles down the sides.

From this it is evident, that new ink consists of at least two substances, one soluble in water, and communicating to it a dark blue colour, the other an insoluble black powder.

A soluble blue and insoluble black substance;

* This was the green vitriol of commerce boiled on iron filings to deprive it of any excess of acid, and to bring it to a minimum of oxigen; but I do not know whether it was exactly in that state. The ink is of a blue colour, and passes through the filter without leaving scarce any residuum: the writing written with it is at first excessively pale, but gradually becomes black.

It

both probably tannates of iron.

It is highly probable, that these are tannates of iron, differing merely in the proportion of oxigen which they contain, especially as the blue is changed into the black by exposure to the air.

Ink not well understood.

The nature of ink is at present not well understood; but it is not my intention to undertake its investigation, I leave that task to an abler hand. I beg leave, Sir, to conclude, by proposing a few queries for your consideration, and for that of your correspondents.

Queries respecting its nature.

In what do new and old writings differ?

Is the difference in consequence of the particles of the old writing having become more firmly united together by time; or is it on account of their having undergone some chemical change*? If the latter, in what does this change consist?.

Is it the tannate of iron, which has suffered an alteration? or is it the gum, which all inks contain?

Is it there any substance capable of dissolving, without decomposition, the black tannate of iron†?

VII.

Of Violet Purple, and the different Tints that may be derived from it; by JOHN MICHAEL HAUSSMAN‡.

Water not the only menstruum of dyes.

WATER is not the sole menstruum capable of extracting the colouring parts of plants, in order to enable them to adhere to alumine or oxide of iron fixed in any cloth. There are vegetables, as alkanet root, which give out their colouring

* That it is not on account of a chemical action having taken place between the tan and the gelatine of the paper, will appear from this, that unsized paper yields a copy no easier than any other.

† From the experiments of Bouillon Lagrange, which render it probable, that strictly there is no such thing as gallic acid, and from the manner in which ink is generally prepared, viz. by long boiling, which must dissipate the acid if it exist, I have been induced to omit taking it into account.

‡ Annales de Chimie, vol. lx. p. 288, December, 1806.

matter

matter only to alcohol. I shall not attempt to define the nature of the colouring matter of alkanet: it is so readily decomposed by the continued action of heat, even below the temperature of boiling water, that, after it has been extracted by alcohol, it cannot be concentrated by evaporation without being destroyed; so that it is impossible to make any farther use of the spiritous part of the tincture of alkanet, as I have convinced myself, by reducing a certain quantity to one fourth by distillation. The alcohol that came over appeared to me perfectly pure; and the residuum was muddy, and unfit for dyeing. I confess I was to blame for not having examined it more thoroughly, to see whether it contained any thing oily or resinous; but I had then no other object in view, than to avail myself of the colouring properties of alkanet, with which I had reason to be satisfied.

Its colour decomposed at a low heat.

On mixing a sufficient quantity of spirituous tincture of alkanet with six or eight parts of pure water in a copper boiler; and afterward dyeing in it hanks of cotton prepared for Adrianople red, according to my process inserted in the Annals de Chimie, year 10, by Mr. CHAPTAL, at that time Minister of the Home Department; at the expiration of an hour, raising the fire gradually till the bath was brought to boil, they were of a fine violet purple colour. To produce this colour constantly of the greatest brightness, the cotton must not be made dull by the preliminary preparations, and consequently must not be galled. The linsed oil I employed for the preparation was boiled with ceruse, taking care not to burn it, that it might not soil the cotton.

Tincture of Alkanet, with 6 or 8 parts of water.

Gave cotton, prepared for Adrianople red,

a fine purple,

The great lustre of this violet purple on cotton, which surpasses that of the finest satin dyed in the common manner, suggested to me the idea of producing it in fine printed goods. My expectations were so far answered with success, that we presently manufactured some whole pieces of long shawls, with a ground of this colour, for Mr Soehné, Sen. and Co. of Paris, who received them a few years ago, and admired them very much. They found the price, however, too high for the times. Formerly, when it was common for ladies of fashion to wear printed calicoes both in summer and winter, it was necessary for those who would force a business to have articles

of a lustre superior to that of satin,

but too expensive for the present times.

cles of this kind of very high price. I had proofs of this two and thirty years ago, when I lived at Rouen; for having then a few gown pieces of ten ells, of a very rich pattern, to sell on commission, I disposed of them without difficulty at two and thirty louis a piece. These articles were from the manufactory of the illustrious John Henry von Schule, of Augsburg, who is well and justly entitled to be styled illustrious, as the first manufacturer in Europe, who carried the printing of calicoes to great perfection and extreme beauty. His articles have made so much noise in all parts of the mercantile world, that the emperor of China desired to see them, and admired them in comparison with the productions of his own dominions.

In 1775, rich prints sold at Rouen for £1 13 6 a yard.

Mr. von Schule the first manufacturer of printed goods in Europe.

Cottons intended to be printed with violet purple grounds, and to have any white figures, require to be very well bleached, that they may be muddied as little as possible in dyeing: for, though the violet purple is such a fixed colour, as to support the action of the alcaline lixivium of oxigenized muriate of potash, without being much weakened, the white is restored but slowly.

Cottons must be well bleached.

Alumine fixed in the cloth, and saturated with the colouring particles of tincture of alkanet, will still admit the colouring matter of other vegetable or animal substances; which gives rise to an infinite number of other tints, that may be increased indefinitely, by more or less diluting the acetate of alumine employed in the printing; and by dipping the violet purples, and their derivative tints, thus produced, in a bath of madder, cochineal, kermes, brazil, weld, quercitron, &c. By mixing these drugs in different proportions, the tints may be greatly increased in number; and still farther by mixing more or less acetate of iron with the concentrated or diluted solution of acetate of alumine.

Other colours may be dyed upon the alkanet, so as to vary the hues.

Cotton printed with oxide of iron, or a concentrated solution of acetate of iron, takes a greenish black from the tincture of alkanet: and by diluting the solution of acetate of iron in different proportions, we shall obtain a great variety of grays, more or less deep, and more or less green. These tints are equally susceptible of variation by means of the dyeing drugs already mentioned.

Alkanet with oxide of iron.

If we wish to produce other colours by the side of the ground

Other colours printed by the side of it.

ground of purple violet, or its derivative tints, without perceptibly altering this ground, it is necessary, before the blocks with other mordants are applied, to pass the alkanet ground through dilute sulphuric acid, to carry off the alumine, that has been left untouched by the colouring particles of the alkanet. The purple and its derivative tints will be reddened a little indeed by the action of the acid, without however being much weakened.

Linen prepared in the same manner as cotton presents nearly the same colours and tints when dyed with tincture of alkanet; and admits the same variations by means of other colouring drugs, or the acetate of iron. Linen may be dyed with alkanet,

The same may be said of silk properly alumed. It affords very brilliant colours by being passed through tincture of alkanet; which however only gives the silk a muddy tinge, if it be prepared with a solution of tin of any kind, instead of being alumed. This shows the little affinity of the oxide of tin for the colouring particles of alkanet, which produce no better effect on linen or cotton, prepared with solutions of the salts of tin. and silk, but not with solution of tin.

The same inconvenience would probably take place with wool, which I have not treated with tincture of alkanet: but no doubt it would exhibit nearly the same colours as cotton, linen, or silk, after having been well alumed. Woollen.

VIII.

On Cast Iron; *by* Professor Proust.*

GRAY and black cast iron afford an aromatic hidrogen, which appears to me to hold in solution a part of the oil that is formed during their solution in acids. This hidrogen burns heavily; and its flame is tinged with yellow and green. Four inches of this gas, however, burned with eight of oxigen, consumed only two, or no more than pure hidrogen would have done. The residuum did not render lime-water turbid. I suspected, therefore, that the oily particles might Aromatic hidrogen from gray and black cast iron.

* Journal de Phisique, vol lxiii. p. 463, December, 1806.

have escaped combustion; but we must not forget, that very small quantities of carbonate of lime are soluble in lime-water.

Six of this and 16 of oxigenized muriatic gas reduced by mixture to ·5

Six inches of this gas, and sixteen of oxigenized muriatic gas were reduced in the space of an hour to half an inch, the greater part of which was still oxigenized muriatic gas. A cloud was formed at the instant of mixture, and a light greasy pellicle floated on the surface of the water, but I was not able to examine it. This gas likewise contains phosphorus. Phosphorus, in fact, must occur in cast iron oftener than is imagined; for I have perceived a phosphate in almost all the solutions of our Spanish cast iron. But beside the ore there are some kinds of charcoal that contribute to this. That of the evergreen oak, for instance, must contain either phosphorus or a phosphate, since the latter is found in its ashes.

Greasy pellicle formed.

Contains phosphorus.

Phosphate in the ashes of the quercus ilex.

Their Plumbago.

Carbon separated from cast iron appears like plumbago.

Is plumbago a carburet?

The carbon separated from cast iron has the leaden appearance, lustre, and scaly texture of plumbago; particularly when it has been thoroughly freed from iron by the muriatic acid: but is plumbago in fact a combination of iron with carbon, a metallic carburet, as it has been considered ever since the time of Scheele? His own experiments, in conjunction with some particular facts, lead me to doubt this; and I am at present fully persuaded, that, before we give implicit credit to this combination, it would be proper to subject it to a fresh examination.

Supercarburetted Cast Iron.

Cast iron overloaded with carbon by long fusion.

I had occasion to examine some cast iron that had been refined according to Grignon's principles, or by keeping it a long time in fusion. The cannons made of it were proved by the corps of artillery under the reign of Charles III, and would not stand the trials.

Its appearance and properties.

This iron, when broken, had not the granulous appearance of gray cast iron: it exhibited to the eye a heap of small needly cones, very obtuse, between which micaceous scales of plumbago were visible when inspected with a lens. The superabundance of this facilitated the crystallization. Under the hammer it is compressed, and crumbles. The file cuts it

it very easily. A skilful workman succeeded in forging a piece without melting it, and formed a plate of it, which, after tempering, appeared to be very steely. Hence I conceive it follows, that, if cast iron gain in metallization by continuing the heat, it loses by the diminution of its oxide a principle, that seems indispensable to the solidity of its texture.

If this oxide, which serves the purpose of interlacing the metallized parts, and of preserving a more complete contiguity between them, happen to be deficient, the liquidity of the fused mass cannot avoid being diminished, and its place must be supplied by carbon, to keep up this effect. But when the iron owes it liquidity to this new principle, it is far from having the same coherence or tenacity as in the former case. Whatever may be thought of this opinion by those metallurgists, who are engaged in casting artillery, I conceive it will be of use to them, to preserve the history of these facts.

Tough cast iron, a solution of oxide of iron in the metal.

But if we continue for the present to consider the carburet of iron as an actual combination, we must allow, that its existence, or its solution in cast iron, affords us an example of a combination, a compound with the excess of one of its elements, or, if you please, of another kind of union, to which Mr. Berthollet does not appear to me to give a full assent.

If a carburet, still a compound united with an excess of one of its elements.

I have examined cast iron obtained with the pit-coal of Asturia in furnaces, that had neither the height, nor strength of blast, commonly required to reduce iron ore by this combustible. This cast iron, on coming out of the crucible, boiled till it began to fix. The result was white, blistered masses, fit for nothing but making cannon balls. It was easy to see, that this ebullition was nothing but a continuation of the effervescence, which had not terminated in the furnace.

Cast iron imperfectly reduced by pit coal.

Its solution confirmed this opinion, for it afforded infinitely less hidrogen than white cast iron.

Afforded less hidrogen.

The labours of Bergman, Berthollet, and many other scientific men, confirmed by the methods practised in England, to promote the disoxidation of the parts in which this process has not taken place, scarcely admit of a doubt, that cast iron is nothing but metallic iron serving as a menstruum to a portion of its oxide. But are not such solutions so many examples of compounds dissolved in an excess of one or other of their elements?

Cast iron retains oxide in solution.

Analogous instances. Sulphurate of copper dissolves in copper, one of its elements. The black coppers contain it, and even sulphurate of iron and sulphurate of silver likewise. We may presume from these instances, therefore, that there would be nothing extraordinary in finding sulphurets, phosphurets, and carburets, dissolved in their respective metals; and consequently, to see these metals dissolve other oxides. If cast iron be an instance of this, that of the oxide of copper in its metal is another, for which we are indebted to Mr. Chenevix; and the experiment of Fernandez on the solution of muriate of silver in its metal is a third.

Copper.

Silver.

Superphosphurretted carbon. Phosphuret of carbon is a compound, that dissolves in phosphorus, one of its elements, in we know not what proportions.

Amalgams. Amalgams are compounds, some of which appear to be proportional, and others not. Some separate from the excess of mercury, and afford means of studying them: others remain in complete solution in it in progressive quantities, the extent of which is not known.

Water. Water is a compound, which by the assistance of circumstances dissolves in oxigen gas, and in hidrogen gas, or in either of its elements.

Hidrogen. Hidrogen is an element of fat oils, volatile oils, camphor, &c.; but we find, that, during their passage in vapour through a red hot gun-barrel, hidrogen can disengage itself from the coal, and dissolve a part of these vapours.

Hidrurets. Nothing surely can be objected to our considering, by an extension of these principles, solutions of sulphur, phosphorus, carbon, arsenic, zinc, &c. in hidrogen, not as simple solutions without any measure, but as so many compounds in due proportion, as so many hidrurets of sulphur, phosphorus, &c. which an excess of the solvent may hold in solution.

Various compound solutions in chemistry. If we cast our eye over the whole field of chemical science, we shall discover there too a multitude of compounds, which dissolve others; some in proportions that are easily estimated by a separation of the excess; and others, that, not yielding to this method, continue to fluctuate in the ocean of indeterminate quantities: so that even at the present hour we are ignorant, whether we ought to place in the same line the

the compounds that are confined to constant proportions, and those that are subject to none, though both are the result of the same power.

Mr. Berthollet, in his Third Series of Inquiries concerning Affinities, expresses himself thus:

Objection of Berthollet.

"Proust asserts, that compounds, the proportions of which are fixed, may unite with an excess of one of their elements in an indefinite proportion; without defining the characters, that distinguish combination from this other kind of union. It is obvious, that, in consequence of the latter distinction, it would be difficult to object to him any observation, which he would not find means to explain."

Proust's answer.

If the preceding facts, to which many others might be added, since the works of the elder chymists are loaded with them, sufficiently prove the existence of these kinds of union, or solutions of compounds by their elements, or even by other compounds; it would appear to me superfluous to insist longer upon them: but I have been able to make them concur in the explanation of certain phenomena, without any contradiction of principles. As to the characters that distinguish them, or ally them to those compounds that range under the laws of proportion, I am entirely of Mr. Berthollet's opinion. But how should I define those characters? All the elements of such unions are not sufficiently known. Chemistry not having yet called for their being subjected to a particular study, it is enough for the present to exhibit them as incontestable facts, till reflection determines their proper place in the edifice of science.

Hidrate of Iron.

A fine yellow ochre, containing a little carbonate of lead.

Mr. William Talaker, our collector for the Cabinet of Madrid, found a very fine yellow ochre in the mountains of Artana, in the kingdom of Valentia. It contains a little carbonate of lead, though there is no mine of that metal in the neighbourhood. This was taken up by weak nitric acid, without altering the colour of the mineral.

On distillation gave out 12 of water.

This ochre, freed from lead and carefully dried, was subjected to distillation in a retort of ten inches capacity. The aqueous vapour that arose completely expelled the air from the retort, and with it about half an inch of carbonic acid

gas

gas. A hundred parts of the ochre freed from lead were reduced to eighty-eight of a pretty fine red powder. The product was pure water.

Muriatic acid separated 44 of sand.

Muriatic acid applied to the residuum, separated forty-four parts of sand: consequently there were forty-four parts of red oxide likewise.

Hidrate of red oxide.

If forty-four parts of this oxide were combined with twelve of water, one hundred parts must have been united with twenty-seven in this oxide. It was therefore a hidrate, with base of red oxide.

If the red oxide, which is generally less disposed to enter into combination, be capable of producing a hidrate, must not the black oxide be much more so? The hidrate of iron then with base of oxide at a minimum will be found some day, either pure, or in a compound of this metal. I am of opinion it is in this state, that it makes a part of the carbonate of iron, the base of which is always at a minimum.

Hidrate of black oxide in carbonate of iron.

IX.

On Filtering Stones, and the Method of determining the specific Gravity of Substances with large Pores. By Mr. GUYTON *.

Filtering stones supposed to be arenaceous.

BOTH Linneus and Wallerius have spoken of a filtering sandstone: *cos filtrum particulis arenaceis æqualibus, aquam transmittendo stillans: cos particulis arenaceis puris aquam transmittens.* On their authority most mineralogists have classed these stones among the varieties of arenaceous quartz: but we do not find, that they had ascertained whether the silex in them were pure, or merely the predominant principle. The former appears however to be the most general opinion, since, excepting one passage in Kirwan, where he mentions the *pierre de liais* among the silicicalcareous stones as porous, and used for filtering†, we do not find in the

Kirwan mentions one partly calcareous.

* Annales de Chimie, Vol. LX, p 121. Nov. 1806.

† Elements of Mineralogy, Vol. I. p. 102.

most

most modern works on mineralogy any mention of that kind of stone, which is so much used at Paris for filtering water, and the advantages of which have been confirmed by an experience of more than thirty years.

Common filtering stone of Paris.

This is a yellowish free stone, of a middle-sized grain, soft enough to be cut with a toothed saw, easily admitting its grain to be rubbed out by the fingers, and yielding a fine powder, by rubbing two pieces against each other.

Spec grav.

I found its specific gravity to be 2·322. A piece, weighing while dry 102·155 grammes, weighed 114·5, after it had lain ten minutes in water, though it was carefully wiped; which gives an increase of 12·545 grammes, or very near an eighth of its weight.

Effervesced with acids.

A hundred decigrammes of this stone dissolved slowly in diluted nitric acid; and the carbonic acid gas evolved occasioned a diminution of weight of 33·59, including the small quantity of water, which it always carries off with it.

The filtered solution left only 12·11 of siliceous earth.

The lime precipitated by sulphate of pot-ash gave 139 of sulphate of lime.

Hence we may deduce the composition of this stone, consisting of

Its component parts.

Carbonate of lime	87·89
Silex	12·11
	100

Place where found kept secret.

I was desirous of knowing the place where this stone was found in strata of sufficient extent, to supply the shops that work it up into filters for water; but from all my inquiries, and what information I could get, it appeared, that the inventor of these filtering stones, who has thus rendered a real service to society, has thought proper to keep the knowledge to himself.

Many similar stones used in building.

In consulting the description given by Mr. Brisson, however, in his Treatise on Specific Gravity, of the stones used for building in Paris and its environs, from the collections of Perronet and Wailly, several are found to exhibit the same characters so completely, that we cannot doubt their possessing

possessing the same properties, and being applicable to the same uses.

There are ten in particular, that resemble it in want of hardness, and size of grain, and that are capable of receiving into their pores from eight to twenty-five hundredths of water: such are, among others, those from Maillet quarries, at St. Leu, and from the quarries at Vergelet, Gentilly, St. Germain, Conflans, St. Honorine, and Bouré, near Montrichard.

The same author mentions, in the series of sand stones, under the name of *filtering sand stone*, a piece from a stone used as a filter, that absorbed a tenth of its weight of water; while the crystallized siliciferous carbonate of lime of Fontainebleau did not absorb quite four thousandth parts; and among the true sand stones, such as those used by paviors, cutlers, &c. and even those in which the remains of organized bodies sometimes occur, there are none that admit so much. These circumstances lead to the supposition, that the specimen subjected to this trial by Mr. Brisson actually belonged to a filter of the same kind as those now generally used; and that he gave it the name of sand stone, merely from the preconceived notion, that the property of filtering existed only in stones of this species.

True sand stones absorb much less water.

We may conclude then, that the filtering stone employed for domestic purposes at Paris is not a sand stone, but a carbonate of lime, containing only 12 or 13 per cent of silex, in such a state of aggregation, as to leave pores sufficiently open to admit water to run out of them gradually as they imbibe it: that it differs not only from the sand stones with siliceous cement, but likewise from the argillaceous sand stones, such as the grindstones of Geneva, Brives, &c. which in time imbibe a pretty considerable quantity of water, but let it pass through with much more difficulty: and that several of the quarries I have pointed out from Mr. Brisson may have calcareo-siliceous strata of the same nature, and possessing the same property.

The proper filtering stone a carbonate of lime, with ·12 or ·13 of silex.

To remove all doubts on this head, it appears to me necessary to offer some remarks on the mode of determining the specific gravity of substances with large pores.

Errours in determining the

It may appear surprising, that I assign to the stone I have described

described and analysed, a specific gravity of 2·322; while Mr. Brisson gives no more than 1·232 for that of the piece of filtering stone, which I have mentioned as serving to establish a similarity. But it must be considered, that, to obtain this result, Mr. Brisson *adds* to the weight necessary to restore the equilibrium, when the substance is immersed in water, the *weight of the quantity of water that has penetrated it.* Such was the method adopted by the author, for substances capable of imbibing water, which appears to me to require a farther examination, though followed by many natural philosophers. It is true Mr. Brisson gives to the specific gravities of these same substances a second expression, derived from a calculation, in which the absolute weight, or the weight taken in air, is increased by the weight of the water absorbed. But neither of these expressions can give the true ratio of the mass of matter to the actual place it occupies; since in the first, that water is reckoned as displaced, which only succeeds to the air that before occupied the pores, and which ascends in bubbles from every part of the surface; and in the second, the weight of the mass is confounded with that of the fluid employed to circumscribe its solid parts.

specific gravity of porous substances,

from supposed corrections.

In fact, if I had proceeded on this principle, I should have found the specific gravity of the filtering stone no more than 1·813, which comes very near to that assigned by Mr. Brisson. On the other hand, if we apply to the data of his experiment the simple calculation of dividing the weight of the body in air by the weight necessary to add to restore the equilibrium when it was weighed in water, we shall have as the quotient 2·391, consequently still a little more than the same calculation for the stone I examined gave me.

But no correction necessary, when the substance is sufficiently permeable.

Sand stones and filtering stones are not the only fossil substances, that receive into their pores the surrounding medium. Chalcedonies, pitchstones, steatites, asbestos, mesotype, schists*, some micas, and even, according to Gerhard, some varieties of jade, are more or less penetrable by water.

Other fossils penetrable by water.

* Mr. Ludicke has described some hard schists, which he found to imbibe and part with moisture so regularly, that they were capable of answering the purposes of an hygrometer.

This

Spec. gravity sought by the naturalist, as illustrating the nature of bodies.

This property unquestionably ought to be noticed in describing them: it makes a part of the characters, the aid of which is requisite to the naturalist in discriminating species; but when he seeks the true specific gravity of any substance, it is to acquire a more intimate knowledge of its nature, not to derive from the measure of a surface full of pores, and roughened with asperities, a gross calculation of the solidity of its mass, as if his object were the estimation of a load.

The problem, which it is of real importance to the progress of science to solve, is to determine the exact ratio, that the proper substance of the body under examination bears to the bulk of its contiguous parts, that leave no more spaces into which the surrounding fluid can have access. The water, which is absorbed as the air escapes, can no more be considered as water displaced by the solid, than that imbibed by a sponge; and we should fall into a great error, if we were to estimate its density on this principle. It would be superfluous to say, that in all cases we suppose the water to have no chemical action, as it has on salts; for then the hydrostatic balance could not even give an approximation to the truth, and we must have recourse to Say's stereometer*; or if we have not this ingenious instrument, which is not yet in very general use, we must employ a fluid that has no action on the subject to be examined, as for instance, water completely saturated with the same salt. Thus I used a saturated solution of nitrate of potash, when I was engaged in the year 11, as member of the committee appointed by the minister at war, to give a comparative table of the specific gravities of all the different kinds of gunpowder used in the fleets or armies of various nations.

Spec. grav. of soluble substances, found by Say's stereometer;

or by weighing in a fluid that will not dissolve, or is already saturated with them: as gunpowder in solution of nitre.

The same principles led me to suspect, a few years ago, the errour, into which most mineralogists had fallen, in ascribing to pumice stone a specific gravity even inferior to that of water. Mr. Klaproth observes, in his analysis of that of Lipari, that, though it contains more than 0·17 of alumine, it is not at all attacked by acids: this, added to the hardness we find in its smallest particles, though they are easily separable, indicate a state of combination inconsistent

Errour in regard to pumice stone.

* See a description of it, Annales de Chimie, Vol. XXIII. p. 5.

with

with the idea of rarefaction attached to such lightness. It is evidently owing therefore to the multitude of pores and hollow spaces, into which the water cannot penetrate, to circumscribe the volume of the solid parts. Powdered pumice stone afforded me a specific gravity of 2·142*; and this is the only method of weighing hydrostatically porous bodies, so as to obtain a constant expression of their density, truly comparable, and affording a just idea of the power of aggregation possessed by their integrant parts, which is the most important point, on which any light can be thrown by a comparison of specific gravities.

Its spec. grav. 2·142.

Porous substances should be weighed in powder.

X.

Report on a Sculptured Head of Flint, with a Covering of Calcedony, made to the Physical and Mathematical Class of the Institute, March 31, 1806. *By Mr.* GUYTON †.

MR. MILLIN, our associate, of the class of history and ancient literature, having had an opportunity of examining a piece of sculpture found in the Faubourg du Roule, thought it his duty to offer it to the inspection of the physical and mathematical class, as an object leading to questions that were interesting both to mineralogy and the arts; and you have commissioned Messrs. Berthollet, Vauquelin, and myself, to make a report to you on the subject.

Antique submitted to the Institute for examining.

Committee.

The fragment was very obligingly entrusted to us by Mr. Cerf, to whom it belongs. It was found, four months ago, in the garden of a house, that was formerly part of the

Where found.

* See Annales de Chimie, Vol. XXIV p. 204.

N. B. I did not neglect this method of verifying the specific gravity of the filtering stone. I reduced it to a fine powder; and the moment it was immersed in water, all the air interposed between its parts, or rather that adhered to its surface, rose in one single bubble; and the loss of weight indicated, without any correction, a specific gravity of 2·261, which differs very little from that I mentioned before.

† Annales de Chimie, Vol. LVIII. p. 75.

Château

Château des Ternes, and is now a boarding-school for young ladies. A gardener discovered it in digging up the ground, at less than two feet deep. This was all the information we could get; and nothing since has been discovered, that can lead to the slightest conjecture respecting the time or circumstances of its being buried there: but the singularities it exhibits sufficiently excite the curiosity of the antiquary, the naturalist, and even the artist, to induce us to attempt to satisfy it, by an examination of what remains of it.

Described

It is a head sculptured out of a piece of flint, of the same nature and appearance as that of which gun-flints are made. From the point of the chin to the crown of the head it is 9 cent. [3½ inches], from the forehead to the back of the head 76 mill. [3 inches], and its circumference, taken above the nose, is 236 mill. [9¼ inches].

A hole 13 mill. [½ an inch] in diameter, in the lower part, and still partly filled with gypsum mixed with lime, appears to have served the purpose of uniting this head with the body of the figure, probably formed of another piece of flint, or perhaps of some substance more easily wrought; and which, according to the usual proportions, must have been 54 cent. [21 inches] high; so that the whole statue would have been 63 cent [24½ inches].

From the form in which the hair is dressed, it appears to be the head of a man. The hair is short, and confined by a simple, narrow band, such as the Greeks and Romans wore; which, added to the style of the figure, seems to indicate an antiquity considerably prior to the times of the Gauls; though the apple of the eye is marked out, which very rarely occurs in really ancient works.

Its age.

But we shall leave to more competent judges the discussion of these points, of which we thought a brief mention necessary, to render the description of the stone complete, and place in its proper point of view the question, that has principally engaged the attention of the class.

The flint covered with a thin coat.

The flint, of which this head is made, has been covered, in all the parts that have neither been broken nor worn away by friction, with a fine white coating of a scarcely perceptible thickness, attackable by no acid, and uniting with a hardness at least equal to that of calcedony the glassiness

of

ıamel, sufficiently transparent to allow the different
)f the silex, more or less gray or bluish, to appear
it in some places.

s covering, for I do not think it can be termed a *crust*, Is this natural or artificial?
k of nature, or of art?

)bvious, that we cannot refer to analysis, to solve this Obstacles to its analysis.
ı; for thus the fragment must be destroyed, and
:n we should not obtain a sufficient quantity of the
; to afford unequivocal results. Nay, should they be
and easy, they could inform us of nothing more,
: know already by its external characters of colour,
hardness, and unalterability in acids, that its con-
parts are the same as those of calcedony.

irst idea that suggests itself on the inspection of this Apparently an enamel.
that the block of flint, after having been laboriously
he wheel, in the same manner as gems, received a co-
n the fire of the same nature as that applied on the
n making porcelain. Not only do the glassiness of
nel, and its thinness, appear to afford grounds for
nion; but it is supported by comparing its shining
with the dullness of the white crust, on two frac-
curring at the bottom of the left cheek, this crust
been formed evidently since it was buried in the

ı large and more recent fracture on the right side ex- But the flint has never been exposed to a strong heat.
:e silex retaining all its ordinary characters; and it is
)wn, that this substance loses its colour and trans-
in a fire incapable of fusing even feldtspar. The
ıt I subjected to this trial was exposed to a heat of
the pyrometer only, when it separated into several
and assumed the appearance of a biscuit to its inte-
ts.

no doubt has led to a more general adoption of the Probably therefore natural.
that the calcedony covering the silex can have been
:d on it only in the humid way, during its having been
round.

:e I embraced it, I thought it necessary to search
collections of minerals of the same kind, for indica-
t least, of the possibility of such a covering being a
production.

In

not preceded by any trial, to adjust the doses of the agents, and the duration and intensity of the fire. The flint was not altered internally; it only acquired a very thin coat on its surface, of a uniform thickness, united into one body with the mass, unattackable by acids, and of such hardness, that it rapidly wore away the stones used by lapidaries, and was as impenetrable to adamantine spar, or corundum, as the coating of the sculptured head.

of great hardness,

The pieces came out of the crucible of a dull white, as I expected; but some parts, which I had polished in the same manner as hard stones, showed them to be capable of as good a polish as the head found at Ternes.

and capable of a fine polish.

It cannot be denied, that such a complete imitation favours the opinion of the coating having been a work of art. It is not necessary for this to suppose, that the chemical affinities, which led to this imitation, were known to the artist who executed the antique: for it would not be the first process found out by loose trials, and practised with success for centuries before the true theory was discovered.

The artist might have employed this process, though ignorant of its theory.

This opinion however has not obtained general assent. Those who contest it rely chiefly on the resemblance of the coating of several flints found in the environs of Ternes, specimens of which were shown to the class by Mr. Chaptal; and which in fact exhibited on some of their faces parts of an enamel, if not equally uniform in tint and in thickness, at least as glossy.

This opinion still questioned

Flints found with a nearly similar coat.

Others have thought with Mr. Fourcroy, that, whether the coating of the sculptured head were formed in the earth, in the same manner as the crusts of these flints, or added by an artificial process after it came out of the sculptor's hand, it must be admitted to have received its polish from art; and that this was the only way of reconciling the inferences we are obliged to deduce from its present state.

Still apparently polished at least by art.

Under these circumstances, the committee can only propose to the class to suspend its judgment, and to leave the subject open to farther inquiry and discussion, for the solution of a question interesting to the history of the arts, and to the sciences of the antiquary and the naturalist.

XL

XI.

Experiments on Double Vision by Dr. Haldat, *Secretary to the Academy of Nancy.**

Vision with a single eye vulgarly supposed to be most distinct.

THE superiority of single vision, or vision with one eye, over double vision, or with both eyes, has long been the subject of two opposite opinions. It is maintained by the vulgar, that single vision is most distinct. Philosophers on the contrary assert, that we see better with two eyes, than with one. The latter opinion, established by father Cherubin in his treatise on distinct vision, and placed beyond all doubt by Dr. Jurin, by means of an experiment, which consists in looking at a sheet of white paper, with a piece of pasteboard, or other opake substance, affixed to the right temple, and projecting so far forward, as to conceal half the sheet of paper from the right eye, while the whole is visible to the left. On looking at the paper alternately with one eye and with both, we perceive very distinctly, that the part seen by both eyes is much brighter than that seen by the left only: the former appears with all its natural whiteness, while the latter appears as if shaded by a thin gauze. Dr. Jurin even estimated the intensity of this obscuration by a very ingenious photometric contrivance.

The contrary proved by Jurin.

If this experimental proof of the superiority of double vision over single wanted farther support, we might adduce the experiments made by means of binocular telescopes, the superiority of which have been acknowledged by all observers, both for distinctness of vision and magnifying power, over single instruments magnifying equally, and of equal clearness.

Binocular telescope.

From this well established fact, that double vision produces a more vivid and distinct sensation than single, it follows, that the sensation produced by the impulse of light on one of the eyes is reinforced, if I may use the expression, by that produced on the other; and that consequently a complex sensation may give rise to a simple perception. But does this faculty of forming simple perceptions, when the impressions are complex, equally take place in all cases? Is it the case when the impressions are heterogeneous, as well

A double sensation produces a single perception.

But may two different sensations be blended into one?

* Journal de Physique, Vol. LXIII. p. 387, Nov. 1806.

well as when they are homogeneous? Such is the question I propose to resolve. I confess, however, that I was led to the experiments, that constitute the subject of this memoir, by an accidental circumstance, on occasion of an eclipse of the sun, the progress of which I set myself to observe. The instrument I used not being furnished with a coloured glass, my right eye was so much affected by it, that it was deprived of distinct vision for some days. When it began to recover, all white objects appeared to me to have changed colour, and acquired a reddish hue, the depth of which I could alter at pleasure, by looking at them with both eyes, or with the affected eye alone. When I looked at them with the right eye, which had been overstrained, they had a red hue; when with both eyes a rose colour; when with the left eye only, white as usual. This fact, the explanation of which is foreign to my subject, led me to conclude, not only that the perception produced by the impulse of homogeneous light on one of the eyes was reinforced by that from a similar impulse on the other; but that the impression of heterogeneous rays on each of the two eyes might give birth to a complex perception, which, being composed of both sensations, would be a mean between the two.

An eye viewing an eclipse without a dark glass, its sight was destroyed for a time, and afterwards represented objects red.

This blended with the natural hue, when both eyes were used.

Desirous of satisfying myself whether the impression of all the primitive colours, applied separately and simultaneously to both eyes, would constantly produce a complex sensation analogous to that I have just related, I determined to procure myself transparent coloured mediums, which, suffering rays of one sort only to pass, might, by being applied separately to each eye, subject this double organ to a complex impression. The difficulty of procuring myself coloured glasses of all the tints, or colours sufficiently transparent to paint similar ones to those of magic lanterns, induced me to reject these, which would have been more convenient, and have recourse to hollow quadrilateral prisms of white glass, into the cavity of which I poured liquids of a proper colour and tint for all my experiments.

Experiments on different colours interposed between the eye and the object.

The choice of tingeing substances for colouring the water, with which the prisms were filled, requires some precautions, of which it may be proper to inform the reader; not

Choice of colouring substances.

not only because all colouring substances have neither the same solubility nor the same transparency, but because they have not all the same tint by reflected and refracted light; as Spallanzani observed with respect to the globules of the blood, which appear red or yellow in the microscope, according to the manner in which they are acted upon by the light. It has long been known, as may be seen in Newton's Optics, that the lignum nephriticum exhibits a phenomenon of this kind. The infusions of violets and litums, which have a pure blue tint by reflected light, have a decided violet by refraction. But it is particularly difficult to obtain the desired tints among the yellows. Those that are the purest yellow by reflected light have a decided orange by refracted. They can only be divested of this red hue, that alters them, by filtering them a great many times, after diluting them with a considerable quantity of water. This effect, which depends apparently on the opacity of the colouring particles, and the force with which they repel the most easily reflected rays, and admit only those that are the least, seems to me well adapted to explain most facts of this kind. In reality it is the least refrangible colour, the red, that generally produces those differences observed in coloured mediums by reflected and refracted light; and these differences are diminished by weakening the tinctures, and diluting them with water.

Many not the same by reflection and refraction.

Blood.

Nephritic wood, violets, litums.

Yellows.

The red rays generally the cause.

Two prisms of glass being filled with different coloured liquors, and applied one to each eye, if we direct both eyes at once to the same object, we receive a double impression, the perception corresponding to which is simple, and that of the colour resulting from a mechanical mixture of analogous colouring substances. Thus a yellow prism, and a red prism, applied one to the right eye, the other to the left, produce the sensation of orange, as a mixture of vermilion and yellow ochre would do. But not to enter into tedious details, I shall give a tabular view of the results of the numerous experiments I made on this subject.

Glass prisms, filled with coloured liquors applied before the eyes.

As the results of these experiments cannot be exact, unless the colours be distinct and pure, in reflecting the light toward the eye we ought to exclude two kinds of bodies, those that are too bright, and disturb the sight by their

Cautions.

glare; and those which, reflecting colours proper to themselves, vitiate the results: such as candles, lamps, &c., which diffuse too abundant light in yellow rays. The best method is to take a piece of white paper, about eight inches in diameter; place it on a black or brown ground at the end of a moderately light room opposite the window, and, standing two or three yards from it, with the back to the window, look at it with both eyes, each having its proper prism before it. In this manner I obtained the following results.

Method of making the experiment.

Results of the combined perception of different colours by refraction.

Red and yellow	produced orange.
Red and orange	aurora.
Red and blue	violet.
Red and violet	a pleasing rose colour.
Red and green	a muddy red.
Red and indigo	an indeterminate colour.
Orange and yellow	light orange.
Orange and blue	muddy green.
Orange and green	light green.
Orange and violet	muddy rose colour.
Yellow and blue	muddy green.
Yellow and green	light green.
Yellow and violet	harsh red.
Blue and green	sea green.
Blue and violet	a deep violet.

The colouring matters that were employed.

In order to enable those who wish to repeat these experiments to execute them with greater facility, and render the results uniform, I shall add here an account of the colouring substances, that were employed. The red was a decoction of brazil brightened by an acid. I likewise employed for this colour red wine, and a decoction of cochineal. The yellow was prepared from quercitron bark, the decoction of which must be weak, well filtered, and brightened: the orange, from French berries, or turmeric: the blue was *aqua cœlestis*, a solution of copper in ammonia: the green, an infusion of mallow flowers changed by potash: the violet was prepared from litmus and violets. These experiments are easy to execute; they only require a little practice, and the habit of distinguishing different tints of colour.

After

After having thus proved, that coloured rays of a different nature, obtained by means of refracting mediums, produce the perception of a mixed colour by their separate action on the eyes; I was desirous of satisfying myself, whether these effects would equally take place from light reflected by different bodies, and received immediately by the eyes. But as it was necessary for this purpose, that the organs should be placed in such a situation, as to render the impression received by each eye totally unconnected with that received by the other, I separated the bodies subjected to the experiment by a thin opake plane, placed perpendicularly between the two eyes. The little apparatus I employed consisted of a square piece of wood, on the middle of which was placed a very thin vertical plane, the upper edge of which was applied against the forehead and nose, so as to separate the two eyes. The whole of this apparatus, which was twelve or fifteen inches high, was painted black in distemper. The coloured surfaces, the double impression of which was to be observed, were placed parallel to each other on the base, one on each side of the vertical plane. These coloured surfaces were little pieces of pasteboard ten or twelve lines square, painted in distemper, and representing the primary colours. It is necessary to have some smaller, and some narrower, and particularly to be provided with at least three shades of each colour.

Experiments with reflected light.

Apparatus.

The apparatus for double vision being placed opposite a window, and the pasteboards on each side of the base, the forehead is to rest lightly on the upper edge of the vertical plane, and then, *viewing both objects with great attention at the same instant*, the effect of the double impression will be perceived. The phenomena that accompany or precede the complex sensation resulting from it are worthy notice.

Method of using it.

Phenomena.

1. When with steady attention, for about half a minute, or even longer if necessary, we see the objects evidently approach each other, and the plane that separates them disappearing, they gradually encroach upon each other, till they are entirely confounded together, if the distance from which they are observed be in proportion to the magnitude of the little pieces of pasteboard; as that of twelve

The objects appear to approach each other,

or

till they begin to overlap; and then blend at once.

or fifteen inches. 2. In this apparent progress of the bodies toward each other, they approach with a pretty regular motion, till they partly cover each other; when we see them suddenly confounded together, as at one leap; and then the compound sensation is changed into a simple perception, and only one single object is discerned, the colour of which is the result of the combination of the colours of the two pieces of pasteboard. All the primary colours, subjected to the same trial, afforded me analogous results, indicated in the following table, a few modifications excepted, which I shall notice.

Results of the combined perception of different colours by reflection.

Red and yellow	produced orange.
Red and orange	a bright aurora.
Red and blue	violet.
Red and green	a rosy green.
Red and violet	a rosy violet.
Red and indigo	a dingy violet.
Orange and yellow	light yellow.
Orange and blue	muddy green.
Orange and green	reddish green.
Orange and violet	light violet.
Orange and indigo	harsh violet.
Yellow and blue	faint, indeterminate green.
Yellow and green	light green.
Yellow and violet	muddy green.
Yellow and indigo	a dingy green.
Blue and green	deep or light green, according to the shade of blue.
Blue and violet	deep violet.
Blue and indigo	deep blue.
Green and violet	a dingy violet.
Green and indigo	a very deep blue.

The experiments require practice and attention, and are fatiguing to the eyes.

These experiments, though not difficult to execute, require a certain practice, and steady attention, without which they will not succeed. The strong convergence necessary to be given to the optic axes renders them fatiguing. I have met with several persons, who, not being able to keep up their attention, and view the two objects steadily at one time, did not experience the compound sensation,

or

or the perception resulting from it: practice however has rendered it very familiar to me, as well as to several persons, whom I have employed to repeat them.

As the perception of the mixed colour in these experiments results from the impression made by two objects of different colours, and received simultaneously by each organ of vision, it would seem, that all colours, being equally capable of producing such an impression, should occasion a sensation equally complete and distinct, and produce it with equal facility. This however is not the case: several of them combine but imperfectly, or not at all. The combination of blue and yellow for instance is not only painful, on account of the continued attention it requires, but the colour resulting from them is vague, nearly indeterminate, and of a disagreeable hue. This singular anomaly, the most remarkable that occurred in these experiments, is not sufficiently accounted for by the extreme difference and heterogeneousness of blue and yellow; since blue and red, which are equally heterogeneous, combine easily and completely to produce a violet. The property of illuminating, which these colours possess in different degrees, confirmed by Newton, and subsequently by Herschel, is the only circumstance, that appears to me capable of giving a plausible explanation of it: for this property of illuminating depends on the force with which the colours act on the eye. Thus when two colours possessing this property in different degrees act at once on the two eyes, the too powerful impression on one necessarily renders that on the other less sensible, and the mixed colour produced by this double impression will not therefore be very distinct. This appears to me the better founded, as the green is more distinct when the yellow is weaker, and the greenish tint produced by the combination of blue and yellow appears to contain much more of the latter colour than of the former: and further, if the impression of the yellow colour be weakened by the interposition of a semitransparent substance, the green is rendered much more determinate. Both yellow and orange combine very difficultly with blue and violet, while they combine together, or with red, very easily.

All colours do not blend with equal ease.

Blue and yellow, painful and imperfect.

This owing to a difference in the property of illuminating.

Hence blue unites best with a weak yellow,

the yellow has more than its due effect,

and the best green is produced by obscuring the yellow.

Homogeneous

Homogeneous colours combine best.

Homogeneous colours of different shades combine with the greatest facility. Thus a strong red and a faint red give a mean tint of red. What appears very astonishing is, that black and white comport themselves as pigments of the same colour would do. White, which is the result of a union of all the primary colouring rays, renders colours lighter, as a mechanical mixture of a white powder would the pigments that represent the other colours. Thus red and white give a flesh colour. Black, which is merely the absence or privation of colour, might be supposed to produce no effect on the organ of sight; yet it has just the same as a mixture of any black powder would with the pigments that produce other colours. Thus light blue or green with black gives the perception of dark blue or green. Glaring colours, such as red and orange, less readily associate with black: but white and black, the white pasteboard being placed on a black ground, and the black pasteboard on a white ground, produce the sensation of gray, like a mixture of ivory black and chalk.

Black and white unite with and modify all colours.

I should tire the reader, if I were to relate all the differences I have observed in colours with respect to the degree of facility with which they combine, or rather associate the effects they produce separately on the two eyes. But I cannot pass over another class of facts, which pertain to the same theory, and may serve to elucidate it. These facts relate to the double vision of objects resembling each other in colour, but differing in form, or differing both in form and colour at the same time. Little parallelograms of pasteboard, either black, white, or of various homogeneal colours, twelve lines long and four broad, placed on the opposite sides of the vertical plane of the apparatus, one parallel the other perpendicular to the plane, exhibit the appearance of a cross with equal arms. Two equal disks, eight lines in diameter, placed on opposite sides of the same vertical plane, are so blended together, that it is impossible to distinguish them. Parallelograms like those just described, or squares of unequal size, if of different colours, exhibit by double vision crosses or concentric squares, the place of junction or superposition of which exhibits the mixed colour.

Double vision of objects differing in figure.

Parallelograms placed in opposite directions form a cross,

and their colours blend where they join.

I shall

I shall not enlarge on this article, as theory alone is sufficient to determine all the cases of alteration of figure by double vision; for it is sufficient to imagine the two objects placed one upon the other, the effect which double vision produces.

In all cases one object seems placed on the other.

I was not satisfied with thus combining the primary colours by pairs only, but extended my researches to more numerous and complicated associations, which led me to some remarkable results, that could not have been deduced by analogy alone from the simple association. The first of these results is, that it is possible to receive at one time two distinct and comparable sensations by the simultaneous impression of several objects on our eyes. Little parallelograms of red, blue, yellow, and green, placed parallel to each other, and on opposite sides of the vertical plane of the apparatus, in the order I have mentioned, and subjected to double vision, give the perception of orange, produced by the association of red and yellow, and of sea green, produced by the green and the light blue.

More than two colours.

Four colours give two objects.

The second remarkable result is, that the colours, in their apparent associations or combinations, seem obedient to a kind of affinity, by virtue of which those that have most analogy to each other combine in preference, be the disposition of the parallelograms what they may. Thus the yellow combines with the red, and the blue with the green, let them be disposed in either of the following modes: yellow, blue, red, green; or yellow, blue, green, red; or blue, yellow, red, green; arrangements in which the colours are obliged to jump over one another, if I may use the expression, to seek out and combine with those analogous to themselves. This tendency of certain colours to combine together in preference to others appears to me to be the principal cause, that opposes the recomposition of light by placing all the primary colours on each side of the vertical plane, which I at first hoped to have effected.

They have a sort of affinity, by which they seek out those most congenial to each other.

This perhaps prevents the combination of all into white light.

The experiments on double vision by means of reflected and refracted light suppose a third class, composed of a combination of the processes of the other two; but, as there is no essential difference in the results, I proceed to explain these phenomena.

The effects of reflected and refracted rays together are similar.

The

Enumeration of the facts.

The facts established by our experiments, and of which we have to give the proof, may be reduced to the following.

1. Simultaneous and separate double vision of objects differing in colour produces a mixed or compound sensation, which gives rise to a simple perception, similar to that which would be produced by a mechanical mixture of colouring substances, representing the colours combined.

2. Objects differing in figure, and similar in colour, combine their figures, as objects of different colours do their colours.

3. Colours in their association, or apparent combination, by double vision, seem to obey a kind of affinity, which renders it more easy between some than between others.

Reason why a simple perception is produced by a compound sensation.

The explanation of the first fact is deducible from the common laws of our sensations. Perception being generally proportional to the sensation, and this to the impression made on our organs, a stronger impression must occasion a more lively sensation, and consequently a proportional perception. Hence when an impression is double, from being received by both eyes, it must be heightened. The perception however will not be double, because we distinguish similar impressions with difficulty; and under the same circumstances, and by the same agents, we are susceptible of impression only to a certain degree.

The facts respecting the apparent combination or association of objects of heterogeneous colours, by artificial double vision, offer several questions to be solved with respect to their cause. The first and chief, with which all the others are connected, is to know why, in these experiments, a double heterogeneous impression does not occasion a double perception, as when we see two objects with both eyes at once; and why on the contrary there is but one perception, as when we see a single object with both eyes.

Why do we not see two objects in this case, as when both are seen by each eye?

Hypotheses of physiologists on single vision.

To explain this common phenomenon of vision, physiologists have invented divers hypotheses, which I have attempted in vain to apply to the explanation of my experiments. Some have asserted, that perception was simple in consequence of a union of the optic nerves, which, being dependant on each other in their functions, could therefore produce

Union of the optic nerves;

produce only a single perception: others, that, as similar impressions cannot be distinguished, we have but one perception, though there are two impressions. But it is easy to see how far these hypotheses are from affording a satisfactory explanation of the phenomena: for it is evident, that, according to the one, we can in no case distinguish similar objects; and according to the other it would be impossible for us to be sensible of the compound perceptions established by our experiments.

and in capacity of distinguishing similar perceptions.

These unsatisfactory.

The explanation of the physiologists who tell us, that perception, being received in a simple subject, cannot be otherwise than simple, by no means elucidates the question. Whatever opinion may be entertained of the cause of our sensations, it is certain, that we have within us a power that tends to individualize, to identify with our being, the different sensations we experience at the same time. Thus in a piece of music we do not distinguish the sound of any one instrument in particular, or the effect of any of the parts that compose it: we receive only a simple perception, resulting from these manifold and simultaneous impressions. Thus two dishes, in which the refinement of luxury has combined substances of the most heterogeneous nature, occasion only a mixed taste, without our being able to distinguish any of those of which it is compounded. But desirous of obtaining a more accurate knowledge of this faculty of identifying and combining simultaneous heterogeneal sensations, I attempted to make experiments on the smell and taste analogous to those on the sight, and this by means of heterogenous flavours and odours of equal solubility and volatility.

as well as that perception is simple, because received in a simple subject.

Faculty of combining into one many simultaneous sensations,

as of sound,

and of taste.

Experiments of this kind on the taste and smell scarcely practicable.

As I was unable to preserve the action of heterogenous agents on these senses sufficiently distinct, to obtain satisfactory results, I attempted analogous experiments on the sense of hearing, which, as it consists of a double organ like that of sight, was better adapted to my experiments. I took two leaden speaking trumpets, covered exteriorly with wet cloths, and introduced their extremities, wrapped round with tow, one into each ear. These were employed to isolate the sounds of two monocords, which I introduced

Experiments on the hearing.

Apparatus.

duced into them, so that the sounds excited in one of the organs were kept distinct from those excited in the other. On making the strings of these little instruments vibrate at the same time, I convinced myself, that the different impressions produced by different sounds combined in the same manner, as when they are received simultaneously by the same ear. The monocords tuned to thirds, fourths, or fifths, to each other, produced the perceptions corresponding with those concords. I know not whether it were prejudice, but these concords seemed to me better united, and more harmonious, that when received by the same ear.

Sounds impressed on each ear separately combine.

These facts, and many others of a similar nature, leave no doubt, that we enjoy the faculty of identifying or combining heterogeneous sensations. However, as we can likewise experience a distinct perception of heterogeneous and simultaneous impressions, it is evident, that this faculty is not so inherent in our organization, as to be independent of certain circumstances, without which we cannot produce the effects that characterise it. Thus we cease to experience the perception that results from the apparent combination of heterogeneous colours, when the objects are at too great a distance from the vertical plane that separates them, or of too great extent.

This faculty dependant on circumstances.

Instance in colours.

These circumstances well considered have enabled me to reduce these facts to the common laws of vision, according to the theory of Buffon and Condillac, which is that of the majority of physiologists. Objects according to these philosophers appear to us single, though there is a double representation of them, one at the bottom of each eye, because the touch, which corrects the judgment formed by the sight, teaches us, that the object, which we see double, is notwithstanding single. Habit and experience have rendered this manner of seeing so necessary, that it is impossible for us to change, without disturbing the order established between the sight and touch. But as every object, that produces a double impression on the retina, is necessarily in the point of meeting of the optic axes, and consequently painted on corresponding points of the retina; and that it is with respect to this correspondence of

We see objects single at first because our judgment is corrected by the touch,

afterward from habit.

the

the images on the two retinas, that the habit of judging an object seen double to be single is established; an object will appear double, whenever this correspondence does not take place. This happens when we press upon one of the eyes in such a manner, as to determine the impression of the image of a single object to parts of the two retinas that are not commonly acted upon simultaneously. This happened, as Cheselden relates, to a man who had one of his eyes distorted by a blow; and he saw objects double, till habit and experience had done for the new points of correspondence on the two retinas, what they had before effected for the points that corresponded previous to the accident.

Hence a single object appears double when the correspondence of its impressions on the eyes is altered, till the new correspondence is learned from habit.

Double objects will appear single on the contrary, when, their impression being made separately and simultaneously on corresponding points of the retinas, we experience the same impressions, as would be excited in us by the picture of a single object. This is what takes place in our experiments: for the two eyes, in consequence of the vertical plane that separates them, receiving separately different and simultaneous impressions on points of the retinas which are the same, or nearly the same, as those that correspond by habit to the two images of a single object, must excite in us the idea of the presence of a single object only. What convinces me that this association or combination of the double image depends on its being depicted on corresponding points of the two retinas is, that, when this correspondence is destroyed by placing the objects at too great a distance from each other, or by giving them too great extent, the association does not take place, and we have a distinct perception of the images of both objects. This in fact must be the case, for the rays proceeding from one object to both eyes necessarily observe certain proportions of correspondence, which cannot exceed a given limit.

Hence double objects appear single by means of this correspondence, as in the preceding experiments.

The superposition and apparent motion of the two objects one toward the other is an illusion produced by the force of habit, which, having constantly taught us, that objects, the double image of which is painted on the bottom of our eyes, without exciting a double perception, are placed in the point of meeting of the visual rays, transfers them to

The motion of the objects toward each other an illusion.

13 minutes, Mr. Berger entered it at - - - - - - - 3h. 41m.
left it at - - - - - - - 3h. 54m.

loss near 11 oz. Weight of his body when he entered - 105lb. 2oz.
five minutes after he came out - 104lb. 7oz. 6gr.

Other effects. On entering he felt a slight burning heat at the nostrils and round the nipples. The perspiration, which began to appear on his forehead in four minutes, flowed copiously from all parts of his body two minutes after. At coming out he felt a little weakness and even faintness. His pulse the instant before beat 128 in a minute.

Experiment III.

From 228° ·8 to 225° ·5, Temperature of the room at the beginning - 228° ·8.
at the end - - 225° ·5.

7 minutes, Mr. Berger entered it at - - - - - - - 4h. 32m.
left it at - - - - - - - 4h. 39m.

loss 7oz. 1½gr. Weight when he entered - - - - - 103lb. 15oz. 3gr.
ten minutes after he came out - 103lb. 8oz. 1½gr.

Other effects. He felt a pretty sharp burning round the nipples, at the nostrils, and even all over the face. In four minutes he was covered with a copious and general perspiration. When he came out he was weak and ill. A moment before he was not able to count the beats of his pulse. Three quarters of an hour after he came out he had recovered his natural state.

Capacity of enduring heat differs greatly in individuals. These experiments agree entirely with those reported by Sir Charles Blagden in showing, that man is capable of enduring exposure to very high degrees of heat for a short space of time: but they show likewise, that there is a great difference between individuals with respect to this capacity. In these experiments Mr. Berger supported heat much better than I, as appears obvious on comparing them. On the other hand, if we compare them with those of the English philosophers, we shall find, that they, or at least Sir Charles Blagden, suffered much less from the heat than Mr. Berger himself. In fact in one experiment he endured for eight minutes a temperature between 240° and 260°, without experiencing more inconvenience, than Mr. Berger did after staying seven minutes in a temperature of 227° ·8. Another time Sir Charles Blagden was exposed for twelve minutes to a heat of 222°, without suffering any inconvenience but a little weariness.

Mr.

Heat applied in various ways.

Mr. Delaroche made a great number of experiments too on the heat both men and animals are capable of enduring in baths of hot water, and in vapour baths; on the influence heat exerts on respiration; on the connexion that exists between the evaporation of the perspirable matter and the faculty animals possess of producing cold; on the influence of heat on respiration; and on the state of the bodies of animals destroyed by excess of heat.

He concludes his labours with the following observations.

The subject not exhausted.

Such are the inquiries I have made, with a view to investigate the effects of a strong heat on men and animals. I could have wished to have extended them farther, and rendered them more complete: but the time such a labour would have required, and the difficulty attending it, did not permit me. I cannot therefore deduce from them general consequences, but I shall briefly recapitulate the results they afforded me.

General results.

Small animals destroyed at 144° ·5, or even at 133° ·25.

1. The object of my first experiments was to determine the degree of heat requisite to destroy animals; and from them I learned, that small animals perished on exposure to a heat of 144° ·5, or even of 133° ·25. It is even probable, that a lower heat, but longer continued, would produce this effect.

Extent of the capacity of sustaining heat differs.

2. Mr. Berger and I confirmed by experiments made on ourselves, the faculty that man possesses of enduring exposure to high temperatures, though but for a short time it is true. A comparison of these experiments with each other, and with those of Sir Charles Blagden, taught us, that the extent of this faculty might be very different in different individuals.

Dry heat borne most easily.

I. The experiments in which we exposed ourselves to the action of aqueous vapour enabled us to verify an observation of Dr. Fordyce, that the sensation of air loaded with vapour is much more painful than that of dry air at an equal temperature.

Perspiration in the ratio of the temperature.

4. We endeavoured to calculate with precision by weighing the effects of heat on perspiration. The loss of weight we experienced in this way appeared to be in the direct ratio of the increase of temperature. We found too, that the heat of aqueous vapour excited perspiration much more powerfully than dry heat.

5. I

24°, immiscible with water, of a pleasant smell, and powerfully reddening vegetable blues. No particular gas was evolved during the operation; the atmospheric air alone being displaced by the gasiform ether. I rectified this ether over potash purified by alcohol; after which it no longer reddened blue vegetable colours, indicated 25° by the areometer, and weighed 420 grammes.

Durosier's with sulphuric acid.

The process by the intervention of sulphuric acid, pointed out by our colleague Durosier, consists in introducing 500 grammes of powdered acetate of copper into a tubulated retort, and adapting to it a Woulfe's apparatus. 500 Grammes each of rectified alcohol and sulphuric acid are then mixed together, and when cold are poured through the tubulure into the retort; heat is gradually applied; and 640 grammes of acetic ether are immediately obtained, mixed with a small quantity of sulphurous acid. This ether marks on the areometer 25½°, powerfully reddens vegetable blues, and forms a precipitate with barytes or lime water. During the process a small quantity of elastic fluid is disengaged, which I found to be sulphurous acid gas.

Examination whether it contained sulphuric ether.

I rectified this ether with 50 grammes of potash purified by alcohol; and, to ascertain whether any sulphuric ether existed in it, I separated what came over into portions of 50 grammes each.

Gravity of the product differed at different periods.

The first portion indicated on the areometer 31°, the second 28°, the third 27½°, the fourth 26½°. These different products together indicated 28°, and weighed 535 grammes.

Gravity of sulphuric and acetic ether mixed near the mean.

To find whether it were easy to detect the presence of sulphuric ether in acetic ether by separating the products, I made a mixture of 50 grammes of the former at 56°, the thermometer being at 0, with 200 grammes of the latter at 25°. The two ethers thus mixed after two days standing indicated 30°.

I distilled about 70 grammes of ether; it indicated 39°, and had the smell of sulphuric ether very perceptibly; whence I concluded, that the mode I had employed was the only one for separating the two ethers.

The two kinds compared.

I afterward subjected the acetic ethers to the following examination.

1. They

1. They were both of a pleasant smell.

2. Their specific gravity differed only four or five degrees.

3. They began to boil at nearly equal temperatures: the first at 50° of Reaumur [144½° F.] the second at 46° [135½°] making a difference of 4°, [9°.]

4. Exposed to the air they evaporated slowly.

5. They were both equally soluble in eight parts and half of water.

6. Sulphuric acid has very little action on these ethers; it colours them slightly; and one part of ether and one of acid, very completely mixed, evolve but little heat, about 30° [67½°.]

7. Nitric acid at 46° is powerfully decomposed by these ethers, a considerable quantity of nitrous gas is evolved, and the residuum is oxalic acid.

From these different facts it follows, that the two ethers are nearly the same, having only some shades of difference, which do not affect their nature.

Thus the process proposed by Mr. Parmentier appears preferable to that of Mr. Pelletier, in being less expensive, requiring a shorter time, and furnishing a larger quantity of ether. But, while I give the preference to this process, I am far from subscribing to the assertion of Mr. Gehlen, that a mineral acid is necessary to the formation of acetic ether.

The process with sulphuric acid preferable, but not essential.

XIV.

Inquiries concerning the Oxidations of Iron; by Mr. Darso.*

It is ten years since the celebrated chemist, Professor Proust, struck with the two combinations that some metallic oxides form with acids, and reflecting on the two proportions of oxigen, that unmetallic combustibles usually take, advanced the opinion, "that metals combine with oxigen only in two proportions:" and though several chemists have since maintained, that there are intermediary

Proust supposes metals to combine with oxigen in two fixed proportions only.

Others the contrary.

* Journal de Physique, Vol. LXIII. p. 292. October, 1806.

oxides;

oxides; and the author of Chemical Statics has gone still farther, asserting, "that the proportions of oxigen united with metals vary, from the point at which the combination is possible, to that in which it has attained its highest degree;" Prof. Proust has not considered the facts objected to his doctrine as sufficient, and persists in the opinion, that nature has fixed these two invariable terms of oxigenation.

Though I consider the subject somewhat differently from the Madrid professor, I have a high opinion of his labours and observations, and incline to think with him, not that the proportions of oxigen are invariably determined by nature, but that most of the facts, on which the opinion of intermediary oxidations are founded, have not all the accuracy such a discussion requires.

But we have not facts of sufficient accuracy to determine the question.

Persuaded, that every research tending to elucidate this point of theory cannot but be of great utility to the advancement of science, I proposed to myself to make some experiments on iron, as one of the metals best adapted to such researches: and I shall relate them in the order I pursued in my labours, persuaded, that I could not adopt a better arrangement, than that of following the ideas that suggested them.

Iron well adapted to the investigation.

The first means that occurred to me for discovering new oxides of iron were, 1st. to treat the red oxide with oxigenizing substances, confining the expansibility of the oxigen by compression. As experiments of this kind relative to the carbonic acid succeeded so well with Sir James Hall, I had no doubt of thus increasing the oxigenation of iron. 2dly, to subject iron wire to different discharges of electricity in air containing more or less oxigen. Previously however I was desirous of ascertaining how iron comports itself in other modes of treatment, to which it has been already subjected.

New oxides might be obtained by the aid of compression,

or the electrical discharge in oxigen.

Oxides by Calcination.

I took one part of iron filings and three of nitrat of potash well powdered, mixed them, and threw them into a red-hot crucible. After keeping up the fire for three quarters of an hour, I withdrew the crucible, and found a great part of the potash and oxide of iron had passed through it. The

Iron calcined with nitre,

much passed through the crucible.

mixture

mixture when cold exhibited a brown mass, with a few green and iridescent spots.

This mass, pounded and washed repeatedly with boiling water, to divest it of its alkali, afforded me a brown powder, strongly attracted by the magnet, and not soluble in cold muriatic acid. Heated with this acid diluted with a little water, it afforded a colourless solution, from which alkalis precipitated a blackish brown oxide, that did not alter by exposure to the air, and at the expiration of a few minutes, had acquired so great a cohesive force, as to be insoluble in cold muriatic acid. When dried in the air it was magnetic, and indeed retained the same characters as before it was dissolved. *

The oxide brown, magnetic, and not soluble in cold muriatic acid.

Dissolved and precipitated, the same.

As the loss prevented my calculating the quantity of oxigen in this magnetic oxide, while its colour and magnetism led me to conclude, that it contained less than the red, obtained by calcining iron filings alone, I proposed to try this method, seizing the moment when the magnetic oxide should be formed. Accordingly I put into a crucible 100 grains of iron filings, and after having kept them half an hour in a brisk heat, stirring constantly, I withdrew them, and found the weight 120 grains. I observed on this occasion, that each grain of the filings, though covered with a stratum of oxide, contained a metallic nucleus; and in order to expose the metal, and accelerate the operation, I triturated these half oxided filings, before I put them again on the fire. On continuing the calcination, and trying the oxide occasionally with muriatic acid, I found the magnetic

Iron filings calcined alone.

* I fancied this at the time to be a peculiar oxide; but I afterward perceived, that its colour and magnetism arose from the concentrated state of the solution and of the alkali with which I precipitated it; since if I diluted the alkali with water, or used lime, strontian, or barytes water, the precipitates were entirely red.

Cause of the colour and magnetism.

On mixing green and red solutions of iron in a certain proportion that may be found by trial, we likewise obtain black magnetic precipitates, that do not change on exposure to the air: but the two phenomena must not be confounded together, for there are magnetic oxides, that do not contain an atom of green oxide. The green salts of iron too may be precipitated so as to be black, magnetic, and unchangeable by the air.

Other magnetic oxides.

Magnetic from ·2 to ·265 of oxigen.

oxide succeed the green, when the 100 grains had absorbed from 30 to 36 of oxigen.

This experiment, which I repeated several times, always afforded me the same results, except that sometimes I found a few hundredth parts of red or of green oxide. It is obviously impossible however, from various circumstances, that every particle of the iron should be equally exposed to the action of the air and the caloric.

Sometimes a little red or green oxide with it.

Red still not magnetic with ·384.

On carrying the calcination so fair, that 100 grains had taken up 38 of oxigen, the precipitates were entirely red, without exhibiting any trace of magnetic oxide. From this term to that of 45 or 50 the oxigenation was very slow, and would have been impracticable without an increase of heat: by raising the fire however, and renewing the air by means of a pair of bellows, I carried the oxigenation as far as 56. This operation is very tedious and tiresome; but an apparatus to save the trouble of renewing the air might easily be contrived, if the excess of oxigen were of any advantage in physic or the arts.

Oxigenation rather slow,

but carried as far as ·36 nearly.

All the oxides after the magnetic alike.

I treated this oxide at 56 with acids, and afterward precipitated it by the alkalis and alkaline earths; I likewise added to its solutions prussiates, gallates, and phosphates; to see if I could discover any properties distinguishing it from the oxides that had preceded it. My trials however were in vain, as I might have expected, since those at 38, 40, 45, and 48 had afforded me no characters to distinguish them from one another.

Redness and magnetism not owing to proportion of oxigen.

The only difference I observed among these oxides was, that the red colour became more decided, and the magnetism weaker, in proportion as the oxidation advanced: but these properties depend more on the difference of the compactness or density of the oxides, than on their proportion of oxigen. *

Different salts of a metal not owing to difference of the oxide.

This conformity of the properties of oxides, among which the difference of the proportion of oxigen extends as far as 20, or even 40 hundredths, as I shall show, proves the error of requiring the formation of a different salt as characteristic of each degree of oxigenation. In fact too extensive an influence on the oxides of iron, and I believe on all the me-

* See the Note subjoined at the end of this Memoir.

tallic

tallic oxides, has been ascribed to oxigen. It has been supposed, that all the properties of oxides of iron, both chemical and physical, are owing to oxigen; while on the contrary, from my observations I am induced to believe, that its part is so passive, as to give it a claim to scarcely any of those properties.

Oxides variable under similar circumstances.

Notwithstanding the uniformity of the circumstances, to which I had subjected the iron in its different calcinations, I observed, that, when 100 grains had taken up 28 of oxigen, the oxide was sometimes entirely magnetic; while at other times, having taken 30 or 32, it afforded a very deep and extremely homogeneous green precipitate; and lastly that at times the red oxide occurred by anticipation at 28 or 30. As I was prejudiced in favour of the less or greater divisibility of substances, on which I believe most phenomena depend, I did not hesitate to ascribe to this cause the results I had obtained: but to satisfy myself on this head, I took 300 grains of iron filings of three different densities, and each portion less dense than that I had before employed. Let us suppose the ratio of their densities to have been at 1, 2, 3. I subjected to calcination the 100 grains of the most dense, removing them from the fire every ten minutes, to triturate them in a mortar. At the end of half an hour, and having been triturated three times, they had taken up 24.70 of oxigen, their colour was become altogether red, their magnetism was very weak, and dissolved in muriatic acid they afforded red precipitates like oxide at 56. I repeated the same experiment on 100 grains of the second degree of density, and when they had taken up 21 of oxigen, the oxide displayed the same properties as the preceding. Finally I subjected to the same proof the last 100 grains, which were extremely fine, and which I had previously sifted, that their density might be more uniform; but instead of taking them from the fire every ten minutes, I triturated them every five, to diminish the action of the oxigen as much as possible. In the space of a quarter of an hour they had taken up 15 of oxigen, and the properties of the oxide were the same as those of the two preceding*.

Most phenomena depend on the divisibility of substances.

Experiments with filings of different densities.

Oxides similar

Thus

* Sometimes they contain 2 or 3 per cent of green oxide, which is not perceived, and which it is difficult to separate, even though it

Red oxide with 15 of oxigen.

Thus we have a red oxide of iron made in fifteen minutes, which gives a fine blue with alkaline prussiates, is precipitated black, or rather of a very deep blue, by galls, and is not distinguishable from the oxide at 56, at least by any of the means hitherto employed for this purpose.

Proportion of oxigen therefore variable.

All these facts prove the sagacity of the learned author of the Statics, when he says, not from elective attractions, but from the properties of oxigen and metals, that the proportion of oxigen to metal may vary from the point at which the combination is possible, to that in which it has attained its highest degree, and that a multitude of circumstances may check or increase the proportion.

I did not carry this experiment farther, but I conceive, that by favouring the division of the iron by all possible means, and at the same time opposing obstacles to the action of the oxigen, we might obtain red oxides of iron with only 6 or 8 per cent of oxigen. And who knows whether by preventing the action of this principle altogether, we might not obtain powders of iron soluble without effervescence in acids, and enjoying the same properties as oxides? For my part I am the more persuaded of it, as I pay little regard to the principle generally received, that metals must be previously combined with oxigen before they will unite with acids. I consider the oxidation rather as a consequence of the means we employ to divide the metals, and reduce them to the degree of fineness required for their solution in acids, than as an indispensable condition of their solution. On this subject I intend to make some researches, and I may then explain the motives that oblige me to question this principle.

Perhaps red oxides with 6 or 8,

and iron soluble in acids without being oxided.

Other metals the same.

To what are the medical virtues of these oxides owing?

Before finishing my report concerning the oxides of iron by calcination, I would wish to make one remark respecting their most interesting application, their medicinal use. It is not yet known, whether these owe their virtues to the iron or to the oxigen; and as the proportions of these in the various preparations of this metal are undetermined, we are wholly ignorant which deserves the preference: this therefore is an object, that merits a careful investigation.

(To be continued.)

it is known to be present. Digestion for half an hour however in very dilute muriatic acid will dissolve it, or rather the iron it contains, without attacking the red oxide.

SCIENTIFIC

SCIENTIFIC NEWS.

Mr. Jessop's Method of blasting Rocks.

THE information respecting the blasting of rocks given in our Journal, vol. IX. p. 230*, has not only been conveyed to France, but the process has been followed there, and different experiments made on it. Mr. Baduel, an engineer employed in executing that part of the road from Simplon, which extends along the south shore of the Lake of Geneva, has availed himself of it on that occasion. He used the common charge of powder there, which is sufficient to fill one third of the hole, and at first filled the rest of the hole with sand. This quantity of sand he diminished gradually, till he found, that two thirds as much as the powder were sufficient. Bran, saw-dust, ashes, and other light pulverulent substances, substituted instead of sand, produced the same effect.

Rocks blasted with sand in Switzerland.

Bran, saw-dust, ashes, &c answered likewise.

Several mines thus charged, and made with various degrees of inclination, in single blocks of stone, and knotty trunks of trees, succeeded as completely, as if they had been stemmed with the greatest care. But the success was not so uniform in the mass of the mountain itself, composed of a blackish siliciferous limestone. In this case the explosion frequently took place without affecting the rock, though the hole was sometimes filled with gunpowder two thirds, or even three fourths of its height †

Failed in the solid rock of the mountain.

These results are scarcely consistent with those obtained by Mr. de Candolle, in the works carrying on over Mount Cenis. This gentleman has seen the blasting with sand executed with success repeatedly, not in separate blocks, but in immense rocks of a micaceous lime-stone schist. Some of these mines, charged with two ounces of powder only, produced as much effect as if they had had the usual charge, which is double that quantity.

Succeeded however on mount Cenis, with a small charge of powder.

* See also, Vol. XII. p. 60.

† The failure in these instances may obviously be ascribed to so large a portion of the hole being filled with gunpowder, the sand being forced through the remaining short space, before the resistance offered by the solid rock could be overcome. The next paragraph confirms this. W. N.

This

At Pesey it succeeded in separate blocks, but not in the solid rock.

This method has likewise been tried at the mines of Pesey. On a separate block it was completely successful; but it was not so in the solid rock. Eight or ten holes 3 cent. (11·8 lines) in diameter, and 3 or 4 dec. (12 or 15 inches) deep, were bored at the same time in the vein itself. Some were charged in the old method, others in the new, putting into each the quantity of powder judged to be sufficient by the workman himself and varying from 6 to 9 decigrammes (2 to 3 oz). All the holes stemmed in the old way blew up the rock; those covered with sand did not even split it.—Some of the latter were charged in the same way with a double or triple quantity of powder, so that it occupied half or two thirds of the hole, the remainder being filled with sand; and the explosion again took place without any effect on the rock. The same holes, which had resisted this double trial, being charged again in the old way with the usual quantity of powder, produced a complete fracture.

Perhaps the effect of this method limited.

From these trials we should be led to conclude, that the new process, though very good for blasting rocks, will not always succeed, when applied to the solid rock of a mountain, which generally presents itself bare but in part; and still less in the interior parts of mines, where the points of contact are more numerous. There seems to be a limit of resistance, beyond which we cannot go by the new method, as we may by the old.

An improvement by Mr. Pictet.

Professor Pictet has proposed another improvement. It is well known, that, in military operations, by making the capacity of the chamber of the mine equal to four times the bulk of the powder employed, a less concentrated explosion is produced, but more destructive at a distance, than if the wadding were in contact with the powder itself.—Muskets too, and cannons are daily burst by leaving a space between the powder and the wadding. Now in working mines, this lateral explosion is exactly what is wanted as strong as possible; it is probable therefore, that it might be obtained with less powder, by leaving an empty space between the wadding and the powder. This method is said to have saved several thousand crowns annually, by diminishing the consumption of powder to that amount, in the mines of the Hartz.

Leaving a vacuity over the powder.

Great saving by this in the Hartz.

Mr.

Mr. Pictet accordingly recommends to those who are engaged in mining a combination of the two methods; one of which, the blasting with sand, would give security to the miners; the other, a partial vacuity, would save powder. His method.
This may easily be effected, by introducing into the hole, after the powder, a cylinder or cartridge of paper, open at one end, and with a small hole in the other, which is to be placed uppermost, to admit the priming straw. Over this the sand may be poured in, and thus a vacuum of two or three inches between it and the powder preserved.

Mr. Gillet-Laumont suggests another additional contrivance. He thinks, if the hole be vertical, or not much inclined from this direction, a more forcible concussion might be given to the rock, by loading the sand with a heavy weight. To effect this he would introduce into the hole an iron cylinder, with a lateral groove for the passage of the straw, and surmounted by a heavy mass of iron, being a continuation of the cylinder. This he supposes would add greatly to the resistance; and the same piece of iron would serve repeatedly for the same purpose, as it could not be blown far, and therefore would easily be found.

Gillet-Laumont's improvement.

A heavy weight over the sand.

Extract of a Letter from Mr. GEHLEN *to* J. C. DELAMETHERIE.*

SIR,

YOU are acquainted with the observations of Sir James Hall on the effects of heat modified by compression †; but Mr. Bucholz has just written to me, that powdered chalk may be converted into a substance analogous to marble without compression.

Wanting to prepare some quicklime, he put four pounds and a half of pure washed chalk into a Hessian crucible, which he covered with a brick, and exposed it in a wind furnace for an hour to a bright red heat, not gradually raised. On examining the contents of the crucible, Mr.

Chalk fused without losing its acid.

* Journal de Physique, Vol. LXIII. p. 238, Sept. 1806.

† See Journal, Vol. IX. p. 98; XIII. p. 328, 381; and XIV. p. 13, 113, 196, 302, 314.

Bucholz found it contracted one sixth. The chalk on the surface and next the sides of the crucible was quicklime to the depth of a line; but this was followed almost to the centre by laminæ adhering strongly to each other; very hard and solid, half fused, and of a yellowish white colour, with a reddish tinge scarcely perceptible. Their hardness was so great, that here and there they would scratch glass; and their softening, or incomplete fusion, which had taken place, was very evident, notwithstanding their laminar form. Under this schistose mass was another, extending to the bottom of the crucible, which bore still more decided marks of fusion. It was broken into seven or eight pieces, which exhibited a perfectly smooth, flattened, conchoid fracture; were so hard in some parts on their edges, as to cut glass, and so solid, as to require a pretty stout stroke with a hammer to break them. Small fragments were in some degree or even quite transparent.

Only one per cent. of its acid expelled.

On dissolving this fused chalk in muriatic acid, it lost 0·42 of carbonic acid, of which before it was heated it gave out 0·43. The acid exhibited itself with all its characteristic properties, and had not undergone the least alteration.

Magnetic iron stone containing red oxide.

Mr. Bucholz has shewn too, that the magnetic iron stone of Suhl in Germany, is iron at a maximum of oxidation, or in the state of red oxide. This appears singular, as it has hitherto been supposed, that the magnetic property is confined to the black oxide, and is destroyed by an excess of oxigen; as it is according to Mr. Hatchett by an excess of sulphur and of carbon, and perhaps of phosphorus.

Volcanic calcedony silex alone.

The same chemist has analysed the hyalite of Frankfort, or volcanic calcedony, and found in it nothing but silex. He had a loss however of 7 per cent. probably therefore it contained also an alkali, which he was not able to examine into, for want of a larger quantity.

Chromat of iron.

Mr. Klaproth has analysed a new fossil from Virieglach in Stiria, which afforded chromat of iron, mixed with a foliated talc in curved laminæ. It was tinged of a cochineal red, and a peach-blossom colour by the chrome.

Flint of recent formation.

Mr. Haquet, of Cracow, the author of several geological works in much esteem, has communicated to me a memoir on the formation of gun flints, and the different

situations

situations in which they are found. He thinks them of very recent origin, since they occur only in calcareous mountains of secondary formation, and near their surface; and besides he has found in these mountains petrified roots, wood, and animal substances. In several fragments he has met with rhomboidal crystals passing in gradation from carbonate of lime to nearly pure silex.

You will find likewise in my journal a paper by Mr. Ritter, concerning the muriatic acid and soda formed by the two poles of the pile. He is pursuing his experiments on this subject; and Messrs. Berzelius and Hisinger are doing the same, for they observed the formation of these in 1802, and consequently before Pacchiani. Muriatic acid and soda formed by galvanism.

Autographs from Stone Blocks.

A method of printing from designs made on stone was mentioned in the last volume of our Journal, p. 158. I am informed, that the circular letters from a snuff manufactory at Offenbach to its correspondents, are printed in this manner; and that a Mr. Reuter, a painter, of Berlin, was the inventor. Printing from autographs on stone.

Art of Swimming.

A society has been formed in Denmark for improving and extending the knowledge of the art of swimming; an art certainly of great utility with respect to health, cleanliness, and safety, and particularly valuable to a maritime nation. Swimming society in Denmark.

Description and Chart of the Faro Islands.

Mr. Loevenoern, a distinguished officer in the Danish navy, has lately published a new chart of the Faro islands. A particular and interesting description of this little known portion of the Danish dominions is given with it. Chart and description of the Faro islands.

Scientific

Scientific Voyage.

Scientific expedition from Russia.

AN expedition has been fitted out from Kamtschatka to the Curile and Aleutian islands, and the North West coast of America, the objects of which are entirely scientific. Mr. Redowski, who accompanied the embassy from the court of Russia to Pekin, as botanist, is placed at the head of it. An astronomer will sail with him for the purpose of making observations, but his name is not mentioned, or that of any other man of science. The voyage is to be of three years duration.

Rockets discharged by Electricity.

Application of electricity to discharge cannon.

THE 14th of February, at two o'clock in the afternoon, M. Bouche made an experiment in the *Jardin des Plantes* at Paris, to try the effect of electricity applied to gun batteries. Instead of guns he had fixed about one hundred rockets on long sticks, disposed in the garden. The rockets were all connected by an iron wire, and the same spark caused them all to explode at the very same instant. The concourse of people was very great, the weather being remarkably fine. This new invention is not intended to increase the destructive powers of those formidable weapons; but it is expected to afford the means of using them without exposing gunners to the fire of the enemy.

Imperfections of Alegar.

Premium for the improvement of alegar.

VINEGAR made of beer, properly called *alegar*, constantly retains a mucous matter, which prevents it from *keeping*. The society of amateurs of sciences and arts at Lisle, wish to have this defect removed; and propose a medal for the best mode of improving alegar in those qualities which may render it equal, or nearly so, to the best wine vinegar. It deserves notice, that this liquor has some properties, which, could they be separated from others not so valuable, would render it worthy of more attention than it has hitherto received.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1807.

ARTICLE I.

A Memoir on two new Classes of Galvanic Conductors. By Mr. ERMAN*.

THE faculty of propagating or isolating electric effects, exhibited in such different and variable degrees by different substances, eminently demands our attention, because the time is arrived for comparing this faculty with the chemical constitution of bodies, to establish something respecting the nature of the electric fluid. The anomalies of the conducting faculty are so strongly marked in galvanic electricity, that they have afforded arguments to those, who refer the phenomena of this class to a principle essentially different from electricity.

Galvanism has been supposed different from electricity, from anomalies in the properties of conducting substances.

The examination to which I have subjected a great number of substances, with respect to the phenomena they present, when they are employed to complete the galvanic circuit from one pole of the pile to the other, has furnished

New experiments answer some of the arguments,

* Journal de Physique, Vol. LXIV. p. 121. Feb. 1807.

To this Memoir the French National Institute awarded the Prize of 3000 fr. [£125], founded by the Emperor, to be given annually to the best paper on the subject of Galvanism, till a discovery on its principle or application shall be made, of sufficient importance to merit the sum of 60,000 fr. [£2500], of which this is the interest.

and lead to a new arrangement of conductors.

me with answers to some of these arguments: but I have obtained a result of much more importance, since I have convinced myself by authentic facts, that in effects of this kind every possible combination is realized; for, if any substance be applied to the two poles of the pile, one of the five following effects will take place.

1. Perfect nonconductors.

1. Either this substance, not acting separately on either of the two poles, leaves them perfectly insulated, when we attempt to set them in action by its intervention. The result of this perfect insulation is, that the galvanic circuit is not completed; and that the electric tension remains at its natural maximum at each pole, without our being able to modify it by the interposition of the substance employed. *Perfect nonconductors* are cold glass, oils, and resins, in every state of aggregation; water, when solid or in vapour; &c.

2. Perfect conductors.

2. Or the two poles exert, through the intervention of the substance applied, a reciprocal action so intimate, that, perperfectly neutralizing each other, every phenomenon peculiar to each ceases, so that it is impossible to act in a distinct and appreciable manner on either of them. *Perfect conductors* are all metals without exception, and in the same degree, at least as far as we know: for it must be observed, that it is only from analogy we ascribe this property to those that have not actually been subjected to experiment; and it is possible, that some metal may have exclusive properties with respect to galvanism, analogous perhaps to those of magnetism and iron. The possibility of this, and the great importance of the discovery, demand a series of experiments, from which we ought not to be deterred by the little probability there is of success.

3. Imperfect conductors.

3. Or the substance applied to the two poles permits their reciprocal action, and completes the galvanic circuit, but in such an imperfect manner, that the distinct effect of each pole will continue to manifest itself, and that it will be possible, by the intervention of the substance applied, to influence each pole separately, according as we act on one extremity of the imperfect conductor or on the other. This property, which I have demonstrated in moist conductors, and in water itself, is so much the more important to be

studied, as it is connected with chemical and physiological phenomena. In fact, except in the case of sparks alone, there is no decomposition that takes place but in conductors of this class; and all the parts of organized bodies, that galvanic electricity is capable of modifying, equally belong to it.

4. Positive conductors.

4. Or the given substance, acting as a perfect conductor when applied separately to either of the two poles, is found nevertheless to belong exclusively to the positive pole, as soon as it is applied to both at once to complete the galvanic circuit. Conductors of this kind do not close the circle completely from their insulating the negative effect; and in the contact of the two poles by their interposition, we can neither charge the positive, nor discharge the negative pole.

5. Negative conductors.

5. Or lastly, the effect mentioned in the preceding paragraph is inverted, that is to say, the substance, that acts on either pole separately as a perfect conductor, belongs entirely to the negative pole, as soon as it is applied simultaneously to the two extremities of the pile. Hence results a maximum of electric tension in the positive pole, and the impossibility of producing any divergence at the negative side by the intervention of substances of this class.

Phenomena of the 1st, 2d, and 3d, already known.

The phenomena of the first and second class have been known too long to excite our attention, though they furnish many interesting particulars. Those of the third I suppose to be equally known; and therefore I shall confine myself to the facts, that demonstrate the existence of conductors of the fourth and fifth classes. These facts, beside their novelty, afford some interesting problems to be solved, and new views to be pursued in galvanic researches.

Must be examined by the electrometer alone.

Before I proceed to the new facts I have to offer, I shall observe, that the phenomena in question do not exhibit themselves with clearness, and in their whole connexion, but as far as they are studied with the assistance of the electrometer alone applied directly to each pole; and without having recourse to the condenser, the employment of which being always interrupted, and its language frequently equivocal, sometimes even deceitful, it fetters the progress of the observation, and never allows us to take it in at one view

Inconveniences of the condenser.

all the changes, that characterise each state of the pile. In the nice experiments I have to relate, it will be seen, that the number of simultaneous observations to be made would render the use of the condenser extremely inconvenient: and if the modifications necessarily produced at each pole by the augmentation of the electric capacity, that results from the very application of the condenser, be considered, the reason of my excluding it will be obvious. Gold leaf electrometers, applied immediately to the poles, and to the the subjects of the experiments, are free from every inconvenience; and if they be ever so little sensible, they indicate with extreme fidelity and promptness the progress and degree of the most complicated modifications, that the pile undergoes.

The gold leaf electrometer very convenient.

Another essential condition to the success of the investigation is, that the pile and all parts of the apparatus be perfectly insulated. I have found no mode of insulating the pile better than to fix it in the centre of a large cake of resin, taking care not to render the cake an electrophorus by any accidental friction. As to the other parts of the apparatus, we should never trust to the insulating power of glass alone; and in applying a resinous coating to the surfaces, I have found the dry way far preferable to the moist. Lastly, before commencing the experiments, and during their course, it is proper to try by means that may readily be contrived and varied, whether all parts of the apparatus completely insulate the electric effects: and it is particularly important, to pay this attention to the electrometers, to be certain whether the glass of these instruments, which cannot be coated with resin, preserve itself constantly in a perfectly insulating state. I know by experience, that the progress of the observations is frequently confused, from the surface of the electrometer having imperceptibly become a conductor. This inconvenience is remedied by drying the instrument, and not by exhausting it, for fear of falling into a still worse inconvenience, the communicating to the glass an electric charge.

The apparatus must be perfectly insulated.

Glass not sufficient.

Particular attention to the electrometer necessary.

Sect. I.

Conductors, that, in establishing a Contact between the two Poles, insulate the negative effect, while they continue to propagate the positive Electricity.

Flame a perfect conductor to either pole separately, but insulates the negative electricity when applied to both at once.

When we apply separately to each of the poles of the e the flame of a spirit lamp, it acts as a perfect conduc- : but if it be applied simultaneously to both poles, it mpletely insulates the negative effect, while it continues conduct the positive electricity with the same energy; and consequence of this partial insulation, the electric circuit not completely established.

Its conducting power disputed

The faculty that flame possesses of conducting the fluid the pile, which has been so much disputed, is placed be- nd doubt by the following facts.

Facts that prove it.

Electricity communicated by flame to either pole.

To either of the poles of a perfectly insulated pile of a ndred pair of plates, more or less, apply a very sensible ld leaf electrometer, which will presently acquire the de- ee of divergence corresponding to the energy of the pile, d the more or less perfect insulation of the opposite ex- nity by the circumambient air. As soon as the diver- nce of the instrument is become stationary, present to the etallic wire of the opposite pole the flame of a spirit lamp mpletely insulated; and the divergence of the electrome- r will not be increased. But the moment a communica- on is established between the flame and the ground, by in- ducing into it a wire not insulated, the electrometer will verge as much as if a communication had been established tween the opposite pole and the ground, by means of an interrupted metallic conductor. This effect is the same the negative as at the positive pole, a circumstance which ll appear by and by of importance. Electricity therefore ay be communicated to either of the two poles of the pile the medium of the flame of spirit of wine.

Abstracted by flame from either pole.

In the same manner it may be radically abstracted from ther of them. Let each pole communicate with an elec- ometer by means of a wire. If an insulated flame touch ther of these wires, the corresponding electrometer will lose

lose nothing of its divergence; but it will be completely deprived of it, the moment a direct communication between the ground and the flame is established.

Both effects may be shown at once by two flames.

These two effects may be seen at once acting in combination, by preparing two perfectly insulated flames, and guiding into each one of the wires proceeding from the two extremities of the pile. If the insulation be perfect in all points, both the electrometers will indicate after a few seconds the same state of divergence, as if the poles were not in contact with the flame. Now if one of the flames be made to communicate with the ground, the electrometer of that pole will immediately lose all its divergence, and the divergence of the electrometer of the other pole will be a maximum. The alternate contact of the two flames therefore produces the same effect, as if we had immediately touched the extremities of the pile itself.

Farther proofs of the conducting power of flame.

Lastly, that we may be fully convinced of flame being an excellent conductor for all the effects of the pile, that do not depend on the closing of the circle, the following facts should be noticed.

Bend the wire on the top of the electrometer, so that the point shall terminate in an insulated flame. Into the same flame insert a wire from one of the poles. If now the opposite pole be touched, the electrometer will receive a maximum of divergence corresponding to the case. If afterward the electrometer itself be touched, the pole with which it communicates through the medium of the flame will be discharged. Lastly, by touching the flame, we shall discharge at once both the electrometer of this pole, and the electrometer communicating with the flame.

Flame therefore does not insulate galvanism, and conduct electricity

These facts prove to a demonstration, that the flame is far from insulating the electric effects of the pile in the cases indicated. They show, that with respect to these cases there is no ground for admitting a galvanic fluid, which the flame insulates, in opposition to the electric fluid, of which it serves as a conductor.

Its conducting power inferior to that of metals.

In the following fact, however, we find an anomaly, which shows us, that the conducting power of the flame, however perfect it has appeared to us in the preceding experiments, is nevertheless very inferior to that of metals, when these

two

two kinds of conductors act in opposite directions. If one of the poles communicate with an electrometer by means of a wire, an uninsulated flame, brought into contact with this wire, will take from it, as has been seen, all the divergence before imparted to the electrometer by the *transient* contact of the opposite pole. But if a *permanent* metallic communication be established between this pole and the ground, the electrometer will reach the maximum of divergence, and remain at it without any diminution, though the uninsulated flame continue to touch the wire, by means of which the electrometer is in communication with the pile. It is to be observed, that this effect is precisely the same at both poles. But how different would be the action of a metallic conductor, if in this experiment it were substituted instead of the flame! It is well known, that the application of uninsulated metal would prevent any intensity of electricity producing divergence; and that the application of a humid conductor would at least diminish it extremely, if it did not reduce it to nothing. Flame therefore, which has hitherto been considered as a good conductor, does not here produce the effect, that was to be expected from it.

It conducts positive electricity, and insulates negative, at the same time.

But this anomaly is of little importance, compared with that which flame exhibits, when it is applied simultaneously to both poles, with a view to close by its means the galvanic circle. The following facts prove, that in this case it belongs entirely to the positive pole, and absolutely insulates all the negative effects, which has led me to place it in a separate class.

Experiment in which it appears a non-conductor.

Let each pole of a well insulated pile, consisting of about a hundred and fifty pair of plates of silver and zinc, be connected with a sensible electrometer. With each pole connect a wire, supported by a completely insulating stand; and let the extremities of the wires be brought so near together, that one flame may be in contact with both. On an insulating stand place a spirit lamp, and commence the experiment by bringing the flame into contact with the two metallic wires. As long as the flame remains insulated, the electrometers of both poles will diverge nearly as if the two polar wires were perfectly insulated. After some time, indeed, the electrometer of the negative pole will exhibit a little stronger

stronger divergence than that of the positive, though every thing else will appear to indicate an absolute insulation: for if a communication be established between either of the poles and the ground, its electrometer will lose all its divergence, and that of the opposite pole will attain its maximum; and on touching both poles at the same time, as strong a shock will be received, as if the two poles were insulated by a stratum of air. It appears, that hitherto philosophers have contented themselves with this single experiment, to affirm that flame insulates all galvanic effects; but the following facts prove, that this insulation is partial, and that flame continues to be an excellent conductor for the positive pole.

Proof that it conducts positive electricity.

Every thing remaining as in the preceding experiment, let a communication be made between the flame and its support; or, which is more simple, touch the flame itself with an uninsulated metallic rod. Immediately all the divergence passes to the negative pole, and the positive is absolutely discharged. If the strongest divergence possible have been previously given to the negative electrometer, by touching the opposite pole, no application of a good conductor to the flame will take off the least part of this negative divergence; while the same application will instantly destroy every vestige of divergence before imparted to the positive pole, and transfer it to the negative side in the strongest degree possible.

Whatever extent be given to the flame, and however near to the negative wire it be touched, it still remains impossible to act through its medium on the negative side, so as to take away the divergence. Flame belongs wholly therefore to the positive pole, since by touching it this pole is immediately discharged, and the negative pole is mediately brought to a maximum of divergence.

This farther confirmed by experiments.

This paradoxical property is confirmed by the following experiments. The two polar wires being united in the same insulated flame, immerse in this flame the hook of a sensible electrometer, and it will acquire a weak positive divergence, if the two poles had not previously arrived at an equilibrium of intensity. But this positive divergence attains its maximum, the moment the negative pole is made

to

to communicate with the ground. If we afterward touch the positive pole, the electrometer immersed in the flame immediately loses all its divergence. Lastly, if a communication be established between the ground and the flame itself, both the electrometer in contact with it, and that applied to the positive pole, are discharged, while that on the negative side attains the highest degree of divergence. These effects are completely explicable, on the supposition that the negative pole is insulated in the flame, while to the positive it is a conductor.

Flame gives out positive electricity to a distance of 1½ or 2 feet perpendicularly,

What renders this absolute insulation of the negative pole by a conducting substance still more paradoxical is the very intimate relation the flame bears to positive electricity. In fact, to take from this pole the divergence that has been given to it, it is not necessary actually to touch the flame; it is sufficient to bring over it, at the distance of a foot and half, or even two feet, a metallic conductor communicating with the ground; when the positive electrometer will immediately arrive at zero, and that of the negative at the maximum of electric intensity. In like manner an electrometer, the hook of which is held at a similar distance above the flame in which the two polar wires of a powerful pile terminate, will very readily become charged with positive electricity, when a communication is made between the ground and the negative side, and will be discharged on touching either the flame, or the pole of which that flame so eminently propagates the effect. This action of the flame extends laterally also, but by no means with equal energy, for in this direction it is confined to a few inches.

and to a few inches horizontally.

Flame does not complete the galvanic circle,

All the indications by the electrometer, that have been related, prove, that the galvanic circle is not completed by the intervention of the flame; and experience long ago showed, that the decomposition of water did not take place, and the physiological effects of the pile were not manifested, when the exciting arc was interrupted by the interposition of flame. Reflecting, however, on the faculty flame has of conducting the electricity of each pole separately, and insulating only the negative effect, it appeared to me possible, to obtain some momentary effects on animals, by discharging at once into the ground the two poles united by the flame,

yet momentary effects on the nerves may be produced through it.

and

and placing very irritable organs in the way of the discharge. After several fruitless attempts, I arrived at the following combination, the success of which has never since disappointed me, and perhaps furnishes an interesting datum for the general theory of the electric charge.

Frequently attempted in vain.

Successful experiment.

Let a powerful pile be perfectly insulated, and its two flames be united in one insulated as perfectly. Prepare as speedily as possible the hinder extremities of a frog, so that the ischiatic nerves shall be disengaged from the flesh, and from the spine, the lumbar vertebræ of which are removed. Place the muscles on the negative pole of the pile, letting the nerves hang down freely; and, holding an exciting arc by a completely insulating handle, apply one extremity to the flame, and the other to the nerves. By this no contraction will be occasioned: or should there by chance be some traces of contraction, as in fact has occurred to me, though very rarely, these must be considered as exceptions produced either by the defective insulation of the handle, a mere mechanical irritation of the very susceptible nerves, or by the action of the atmospheres of the poles; for I have found in another series of experiments, that every pole, charged by the contact of the opposite pole, becomes the centre of a sphere of activity, in which the capacity of substances is powerfully modified without contact, and solely by the mechanism of electric influences.

Cautions.

This may account for sparks being obtained by the contact of a single pole.

I am tempted to explain by the last mentioned property those sparks, which observers of credit affirm they have obtained by the contact of a single pole, when the pile consisting of a thousand pairs possessed very great energy: and I conceive, that the contractions sometimes seen in the case in question result from the weak positive electrisation, which the negative pole produces by its influence on the exciting arc, so that the equilibrium is restored not between the positive and negative poles, to which the flame presents an insurmountable obstacle, but between the negative pole and the anterior part of the insulated arc, become positive by influence. It is obvious, that the effect of this restoration of equilibrium must be of infinitely small intensity; and that, to produce the weakest contractions, it supposes an extraordinary degree of excitability.

Be

Be this as it may, to prevent any mistake from creeping in, if the contact of the insulated conductor, which terminates at one extremity in the flame, and at the other at the nerves, produce a contraction during the period of the highest irritability, a few moments should be suffered to elapse, the application of the insulated exciter should be repeated from time to time, and very soon the application will produce no effect. The experiment then properly commences. In fact, when the insulated exciter has no physiological action, it is sufficient to establish a communication between it and the ground, either by touching it with the finger, or taking it in the hand without the insulating handle, and very strong contractions will be produced every time the circuit is completed from the flame to the nerves. The influence of the ground may be proved, by completing the circuit with an insulated and an uninsulated arc alternately. If a certain interval be allowed between these comparative applications, those with the insulated arc will never produce any effect, those with the uninsulated will constantly excite contractions. I must observe, however, that this kind of galvanic excitement, by the intervention of flame and the ground, requires a much greater excitability in the subject, than the common method of completing the circuit immediately from pole to pole; for the muscles are obedient to the latter, long after they have ceased to contract by the application of an uninsulated conductor to the flame. It is to be understood, however, that, if the prepared muscles be placed on the positive pole, and the circuit then be completed from the flame to the nerves, no effect will be obtained, whether the arc be insulated or uninsulated; for, as the flame belongs exclusively to the positive pole, it is obvious, that it cannot produce contractions with the pole of its own nature.

Farther precautions.

Experiment.

The irritability of the subject must be great.

When placed on the negative pole, no effect is produced.

The explanation of this fact appears to me to arise naturally from what has been said. The flame insulates all negative effect, and consequently cannot complete the galvanic circuit. But in the application of the uninsulated arc between the flame and the nerves, it is properly the ground that serves as an intermediate chain, and the mind may distinguish three different effects at the same instant of time. The first is that of charging the negative pole to a maximum

The fact explained.

at

at the expense of the ground: the second, the returning into the ground all the excess; by which the positive would arrive at a maximum of intensity, were there not a want of insulation: and from this want of insulation results, as a third effect, the momentary discharge of the two poles into the ground. It may be conceived, that very irritable organs, serving as a vehicle to this process, will experience that kind of shock, which accompanies the prompt restitutions of the electric equilibrium. If my object were at the present moment to display a theory of the electric charge, I certainly should not content myself with these germes of ideas, which however appear to me fertile in their consequences. It may be presumed too, that this kind of excitation, in which the ground at large concurs, must require a much greater degree of irritability, than those in which the equilibrium is established immediately from one pole to the other. Whether the impossibility of obtaining chemical decompositions by the intervention of flame depend on this circumstance, I cannot say; or even whether the impossibility be absolute: all I know is, that I have never produced any such effect, notwithstanding the numerous combinations I have tried.

The author could never obtain any chemical decompositions in this way.

When the insulating power, which has been so peremptorily ascribed to flame, be considered, the following observation will appear interesting. To produce the contractions just mentioned, it is not necessary, that the uninsulated exciting arc should immediately touch the flame, as it may be held several inches above it. I have sometimes succeeded in producing contractions, when it has been held a foot and half above it, particularly when I have armed this extremity of the arc with a metallic disc a few inches in diameter, in order to bring it into more intimate contact with the hot air issuing from the flame, and serving as a conductor to the positive electricity.

In this case too the flame acts at a distance.

I shall just mention here another observation, which I have repeated several times, but the particulars of which I am far from having sufficiently studied. When the exciting arc, brought into communication with the ground, has produced a contraction, by being placed simultaneously in contact with the flame and nerves, it will retain this property for about

The exciting arc retains its property some seconds.

about twenty seconds, without its being necessary to keep it insulated during this time. In this state it produces a fresh contraction on touching the nerves alone, without requiring the flame to touch the other extremity. This observation has nothing in it of novelty, as there are many analogous facts: yet it is in some degree interesting, as it facilitates our varying the modes of experimenting. But what induced me to mention it here was, that the success of the experiments, in which an insulated and an uninsulated arc are alternately employed, depends on this circumstance; and for this reason, in describing these experiments, I mentioned the necessity of allowing a certain time to elapse between each of these comparative applications.

Flame of alcohol thus shown to be an excellent conductor of either electricity singly, but to insulate the negative when employed to complete the circuit.

The facts I have recited incontrovertibly prove, that the flame of spirit of wine is an excellent conductor for either pole of the pile; but that in connecting the two poles it completely insulates the negative side, while it continues to be eminently conducting for the positive. But the problem is still far from being solved: it remains to be known, what is the mechanism of the action, on which this singular property depends. It would certainly be rash to determine any thing respecting facts so new, and deviating so widely from all known analogy: I only mention the following hypothesis, therefore, on account of the interesting facts of which I have obtained a knowledge, taking it as a text for farther researches.

Flame supposed in some instances to dissipate electricity: in others to collect it.

I had long imagined, that the electric intensity manifested exclusively at the negative pole by the intervention of the flame might depend on the two opposite properties assigned to it, and in fact distinguished in certain phenomena of common electricity. We conceive we have equally reason to say, that flame dissipates and destroys all electricity, as for instance, when charged plates of glass or resin are presented to it; and that in other cases it collects electricity, as when it is applied to the summit of electrometrical points intended for meteorological observations. I thought, therefore, that something analogous took place here: but the dispersive effect being much superior, the positive pole was constantly discharged by drawing off the excess of the elective fluid, while by this very act the negative side was left

at

But this hypothesis will not apply here.

at a maximum of intensity. But a more accurate analysis of the phenomena, and a further investigation of the facts that occurred, convinced me of the erroneousness of this hypothesis: for if it were by a simple dissipation of the electric fluid, that flame destroys the intensity at the positive pole, and carries it to a maximum at the negative, it must be perfectly indifferent, whether the flame were insulated or not.

Solids produce similar phenomena:

Now we have seen, that this is not the case. Besides, it will appear, that solid substances produce analogous phenomena, though inversely: so that here we have no expansible fluid to dissipate or accumulate the electric.

and flames from different substances differ in their action.

But what demonstratively proves the falsity of the hypothesis is the total difference, that exists in the mode of action of different flames, according to the chemical constitution of the bodies from which they emanate.

Flame not a distinct substance always of one nature.

It is a singular abuse of the abstract signs of language, to speak of flame as one constant homogeneous substance, whatever be the the nature of the matter undergoing ignition. This errour might have been pardonable previous to the discoveries of pneumatic chemistry, particularly with respect to common electricity, the chemical effects of which are nothing, or difficult to ascertain. In galvanism, on the contrary, the chemical effects stand foremost: every physical effect is preceded or accompanied by chemical action; and it is precisely from this, that the discovery of Volta will for ever remain a memorable epoch in the annals of science. His pile is a landmark erected on the common frontier of chemistry and natural philosophy. A comparison of the mode of action of the flame of different combustibles soon decided the fate of my hypothesis.

Electricity and galvanism differ in their mode of operating.

Flame from hidrogen and carbon produces the preceding effects.

All flames arising from the incandescence of substances containing hidrogen and carbon produce the phenomena of insulating the negative pole, and acting as conductors to the positive, in the same degree. Those, on the contrary, that contain neither hidrogen nor carbon, either do not produce this effect; as sulphur, the flame of which equally insulates both poles: or produce an effect totally opposite; as phosphorus, which, in a state of ignition, insulates the positive, and conducts the negative. I shall enter a little into the detail of these facts.

Flame of sulphur a complete nonconductor.

Flame of phosphorus insulates the positive pole only.

On

Flame of various substances.

On uniting the wires of the two poles with the flame of a wax or tallow candle, an oil lamp, yellow amber, camphor, volatile oils, and several other hidrocarburetted substances, the effects I have described, taking for example the flame of alcohol, will be observed fully. I had a strong reason, however, for proposing the latter, since the combustion of the substances here mentioned is scarcely commenced under the influence of the galvanic poles, before a fuliginous deposition takes place on each of the wires, particularly on that of the negative pole. This deposition is distinguished by a kind of dendritic vegetation, very striking on the negative wire, but much less distinct, and sometimes not to be perceived on the positive. These ramifications increase and spread with great rapidity, particularly at the negative pole: they tend toward one another from the negative to the positive, and the moment when these fuliginous filaments fill the space between the two wires, all electroscopic effect ceases, the circuit being closed by the conducting power of the carbon. The flame of spirit of wine, or of naphtha, is free from this inconvenience. If the experimenter would observe these fuliginous vegetations in the greatest energy, he should burn in a small capsule, oil of turpentine rectified by distillation. On bringing into this flame the two wires of a galvanic pile of tolerable strength, the fuliginous vegetations will be produced in such abundance, that frequently they will be seen to rise from the edge of the capsule, and form by their ramifications a very pleasing crown, the incandescent points of the tufts having a very rapid movement of tension on the fuliginous pedicle that supports them.

Soon produced an arborescent soot,

especially on the negative wire,

that stretched from one to the other.

Alcohol and naphtha have not this effect.

Oil of turpentine has eminently,

and forms a pleasing exhibition.

To obtain the partial insulation of the negative effect, it is not necessary, that the substance from which the flame emanates should contain carbon. I filled my gazometer with very pure hidrogen gas, carefully washed; received the flame of a stream of this gas perfectly insulated between the two wires of a pile; and observed, that the effects during the whole course of the experiment were perfectly identical with those which I have described above with the flame of alcohol.

Carbon not necessary.

Hidrogen alone sufficient.

As to the flames emanating from substances that contain neither carbon nor hidrogen, it is very probable, that none of

Of other substances only sulphur and

phosphorus tried.

them produce the phenomena of the partial insulation of the negative effect. With respect to sulphur and phosphorus, I have proved this by experiment; and I am disposed to extend it by analogy to all substances of the same kind.

Flame of sulphur.

The uninsulated flame of pure sulphur, applied to either pole of the pile, acts as a perfect nonconductor. It is impossible to discharge either of the poles by the application of this flame; and the opposite pole shows no increase of intensity by this contact. Hence it follows, that the two wires connected by the same flame of burning sulphur remain equally insulated; and if a communication be established between this flame and the ground, it is still the same. The flame of sulphur, therefore, insulates the galvanic electricity as perfectly as the substance from which it emanates; and consequently the fourth class of effects do not depend on the dispersive property of flame, as flame.

The action does not belong to the flame itself.

Its connexion with chemical affinity shown by additions to the sulphur.

On the contrary, the intimate connexion of these phenomena with chemical affinities is demonstrated, by joining with the sulphur some hidrocarburetted substance. Thus on connecting the polar wires by the uninsulated flame of a match, or of a thread dipped in sulphur, the divergence is null at the positive side, and extreme at the negative.

Flame of phosphorus a conductor of the fifth class.

As to the flame of phosphorus, it exhibits a very remarkable property, in belonging decisively to the fifth class: that is to say, applied individually to each pole, it acts as a perfect conductor; but the moment the two wires are united in it, the positive side is found to be completely insulated, while, with respect to the negative pole, the conducting power continues in full energy. I shall not enter into the particulars of the experiments, as they were conducted precisely in the same manner as those already mentioned: but I shall observe, that, to satisfy myself whether the moisture adhering to the sticks of phosphorus, taken from under water, had any influence on the phenomenon, I several times took the precaution, carefully to wipe the pieces I intended to employ, and then to keep them a whole day in a phial filled with calcined muriat of lime. This perfect desiccation did not affect the phenomena. Neither did brown and opake phosphorus, obtained directly from distilling the acid with charcoal, differ in its effects, or in their degree, from

The experiment not influenced by the phosphorus being wet,

or impure.

that

that which I had brought by subsequent operations to that colour, semitransparency, and fracture, which indicate its greatest purity.

Brown phosphorus perhaps not rendered impure by carbon.

Perhaps we may infer from this, that the impurity of brown phosphorus does not arise, as some have supposed, from a portion of carbon carried over by the phosphoric vapours. In fact, if the smallest portion of carbon, burned with sulphur, immediately communicate to its flame properties absolutely different from those of the flame of pure sulphur, analogy leads us to expect similar effects from carbon incorporated with phosphorus. But I found nothing of this in the combustion of brown phosphorus. I am free to confess, however, that this is an argument of no great weight, particularly as the most essential point of comparison is still wanting, for I have never been able to succeed in burning together phosphorus and charcoal mixed in different proportions.

(To be concluded in our next.)

II.

Facts toward a History of Prussiates. By Mr. PROUST.

(Concluded from p. 109*.)*

Some Precipitations by the Simple Prussiate.

Precipitates with the simple and triple prussiates differ.

THIS prussiate, with the metallic solutions, gives different results from the triple prussiate, some of which had already been noticed by Scheele. The following are those I have observed.

Silver.

Silver, with the triple prussiate, gives a white precipitate, which soon turns blue, in consequence of the white prussiate of iron mixed with that of the silver.

With the simple prussiate it produces a white curd, that does not change.

Gold.

Gold is not affected by the triple prussiate.

With the simple prussiate it gives a white precipitate, that turns to a fine yellow. This precipitate is a true prussiate of gold, and does not fulminate by exposure to heat. Distilled in a retort it gives out water, empyreumatic oil pretty abundantly, and gaseous oxide of carbon that burns with a blue flame. The residuum is gold mixed with powdered charcoal. I find no mention of ammonia in my notes, whether it were forgotten I do not know.

Molybdic acid. Molybdic acid has no effect on either of the prussiates.

Tungstic oxide. Neither has oxide of tungsten.

Titanium. Titanium, with the triple prussiate, afforded prussian blue, in consequence of the iron retained by the oxide.

With the simple prussiate it gave yellow oxide of iron, such as this prussiate produces with solutions of red oxide. I have never yet been able to obtain titanium perfectly free from iron.

Uranium. Uranium gave a blood red precipitate with the triple prussiate. With the simple, a yellow white.

Cobalt. Cobalt gave a grass green precipitate with the triple prussiate. With the simple, a light cinnamon.

Nickel. Nickel gave a greenish white precipitate with the triple prussiate. With the simple, a yellowish white.

Manganese. Manganese gave a peach blossom precipitate with the triple prussiate. With the simple, a dirty yellow.

Copper. Copper gave a fine crimson with the triple prussiate. With the simple, a yellow.

Muriate of copper. White muriate of copper, or that in which the oxide is at a minimum, dissolved in muriatic acid, gives with the triple prussiate a white precipitate, but tinged with a little crimson. It appears, that the precipitate would be white, if the muriate were completely free from oxide at a maximum: but the solution of this muriate is like that of iron, it is difficult to keep it at a minimum of oxidation, in consequence of the action of the air.

With the simple prussiate this muriate gave a perfectly white curdy precipitate. A few drops of solution of potash took from it its prussic acid, and turned it yellow, which is the colour of oxide of copper at a minimum.

Platina. Platina afforded nothing with either of the prussiates: but I find a memorandum, to examine it again.

Prussiate of mercury. Prussiate of mercury is obtained, as is well known, by treating red oxide of mercury with prussian blue. This salt easily crystallizes in tetraedral prisms. It is always opake. It may retain potash, as will be seen presently, if there were any in the prussian blue. It equally retains oxide of iron, as may be seen by the following experiment. Heat a few grains with muriatic acid in a little matrass, and white prussiate will be precipitated.

Freed from iron. To free it from iron, its solution must be boiled with red oxide of mercury repeatedly: each time it deposits oxide of iron, but this depuration is tedious. The prussiate of mercury changes its state by being boiled with red oxide, and appears to take up a surcharge; for it no longer crystallizes in prisms, but in small groupes of very fine needles. Their solutions too require to be farther concentrated; and dissolving the crystals afresh does not bring them back to their original figure.

Decomposed by heat. This salt heated in a retort is easily and wholly decomposed, if the fire be not urged too strongly. It is sufficient to heat a few grains in a tube of three or four lines diameter, closed at one end. If, while thus heated, the open end be exposed to flame, the prussic gas mingled with gaseous oxide of carbon takes fire. The flame is red and blue, terminated by a yellowish aureola. One hundred grains of prismatic prussiate gave one time seventy-two grains of mercury, at another seventy-two and half. The residuum of eight or nine grains was a mixture of charcoal and carbonate of potash. This is nothing extraordinary, for the alkali cannot decompose prussiate of mercury, and no doubt it was contained in the prussian blue, which was that of the shops.

Products. The products that arose in this distillation were ammonia; oil, and this even in tolerable abundance; and a mixture of carbonic acid gas, and carbonic oxide.

The oxide of mercury always at a maximum. There does not appear to be any prussiate of mercury with oxide at a minimum for its base; for the prussic acid applied to mild muriate of mercury, or to the nitrate with base at a minimum, eliminates a portion of the mercury, and produces a prussiate with base of red oxide, like that obtained directly by treating red oxide with the acid.

Red oxide of mercury decomposes both prussiates of potash.

The red oxide of mercury equally decomposes the simple prussiate. The potash too is separated from it; and as this has no action on the prussiate of mercury, the prussiate crystallizes amidst it. It likewise decomposes the triple prussiate completely, but this requires long boilings. In this process, the black oxide in the triple prussiate passes to the state of red oxide, and is deposited as an ochre. Part of the mercury gives out the oxigen requisite for this, and hence it is found in the metallic state among the ochre precipitated; but without this superoxigenation of the iron, which diminishes the affinity of this metal, the oxide of mercury probably would not decompose a combination so solid as that of the triple prussiate.

Prussiate of mercury not decomposed by sulphuric acid diluted,

The aqueous sulphuric acid has no action on prussiate of mercury, even with heat. Not the slightest smell of prussic gas is given out.

Potash saturates the sulphuric acid as a vehicle of the prussiate, but occasions no precipitate.

Concentrated sulphuric acid destroys the prussic, gives out sulphurous acid, and thus destroys all means of comparison.

or by nitric;

Nitric acid is not more successful even with boiling. At the beginning, indeed, a little nitrous gas is perceived; but this, no doubt, is occasioned by the black oxide of iron contained in the prismatic prussiate. The prussiate, however, crystallizes in the midst of the acid; and alkalis saturate this acid without precipitating any thing.

but it is by the muriatic.

But it does not elude the action of the muriatic acid in like manner. There is a separation of prussic gas, a complete decomposition, and the prussiate is totally changed into corrosive sublimate. Accordingly, alcohol dissolves the saline residuum of this process completely; and we find nothing but sublimate on trying it by reagents. It is well known, that alcohol does not dissolve the prussiate of mercury.

Soluble in potash.

Potash dissolves the prussiate of mercury abundantly, by the assistance of heat; and this salt crystallizes in it on cooling. Alcohol separates it too, and it is found entire.

Decomposed by muriate of tin, and hidrosulphuretted water;

Muriate of tin at a minimum, and hidrosulphuretted water, decompose this prussiate instantly, and the prussic acid is set free.

It

It has been seen, that muriatic acid acts effectually on this prussiate: it might be supposed, therefore, that muriate of ammonia, which offers the prussic acid a principle capable of uniting with it, should make a change of bases; but it does not. If a solution of prussiate of mercury be heated with muriate of ammonia, nothing new is produced, and alcohol separates them completely. Potash and limewater precipitate nothing from the mixture, not an atom of corrosive sublimate; and the green sulphate of iron, which would not fail to form a prussiate of iron with that of ammonia, if it met with any in the liquor, does not experience the least change. but not by muriate of ammonia.

Prussic gas.

On heating 1440 grains of triple prussiate in a retort, with a sufficient quantity of dilute sulphuric acid, four ounces [2304 grs.] of alcohol were impregnated with about 80 grs. of gas. I kept the alcohol in a jar in a mercurial trough: the gas dissolved in it rapidly, but it would have taken up much more. The water of the intermediate receiver too was loaded with it: its smell was suffocatingly pungent, and its kernel flavour was extremely strong. This water did not render that of barytes turbid. The gas has a constant tendency to escape, and is perpetually raising up the cork. If a small matrass filled with the solution be immersed in hot water, the gas separates rapidly, and burns at the orifice: on bringing a candle near it, smoke is perceived; no doubt because a part of the charcoal escapes, as in the combustion of volatile oils. Prussic gas.

Prussic acid dissolved in water, and kept in a bottle closely stopped, is decomposed spontaneously. In four or five months it becomes yellow. It gradually loses its smell, grows turbid, and deposits a coffee coloured sediment, which, after being heated, exhibits all the characters of charcoal. Prussic acid decomposed spontaneously in water.

By distillation it affords a little water, prussic acid, and ammonia. The carbon is azotized; and it acquires one of the principles, which the acid relinquishes on being decomposed, for, on heating it with carbonate of potash, it afforded me a lixivium capable of making prussian blue. Decomposed by heat.

But

But while the carbon in separating retains azote, the greater part of the latter, combining with hidrogen, forms ammonia. Thus ammonia is found in the yellow liquor, with the remainder of the acid that has escaped decomposition.

Solution of prussic acid and sulphate of iron.

Prussic acid dissolved in water does not render the solution of green sulphate of iron turbid, till it has undergone the changes just mentioned; it then affords blue with it, from the concurrence of the ammonia recently formed.

Solution distilled.

Finally, this solution being distilled affords prussiate of ammonia, and nothing more is found in it but some particles of carbonaceous matter, which fall down. It would have been of importance to ascertain, whether there were any carbonic acid with the ammonia, but I forgot it at the time. I intend, however, to examine it again.

Solution of the acid in alcohol keeps very well. Inference.

The alcoholic solution keeps perfectly well. Hence we may even infer with some reason, that, as alcohol is better adapted than water both to dissolve and retain it, the prussic gas, considered too with respect to its qualities of being aromatic and inflammable, perceptibly approaches nearer to oily, combustible, and complex products, than to saline substances.

General deductions.

From these facts it follows, first, that there is but one prussiate of mercury, the base of which is at a maximum: secondly, that the augmentation of attractive power, which the prussic acid borrows from the black oxide, when it has to combine with potash, or the red oxide of iron, and on which Berthollet has so justly insisted, ceases to be necessary, when it has to unite with the oxides of gold, silver, copper, cobalt, nickel, uranium, mercury, &c. We see, in fact, with regard to the latter, this acid, the affinities of which are so sluggish, so little adapted to entitle it to the name, has notwithstanding no need of black oxide, to furnish with mercury a saline compound, very soluble, very crystallizable, and, in short, possessed of all the characters, that distinguish the most perfect compounds. To these anomalies let us add those it has of preferring mercury to all the alkalis; not yielding its oxide either to the nitric or sulphuric acid, each of which is so far beyond it in strength; and lastly, to yield it only to the muriatic acid,

which

which is in so many respects inferior to the sulphuric and nitric.

Lixivium of animal charcoal.

Prussic lixivium.

Equal parts of charcoal of blood and carbonate of potash, heated to redness in a covered crucible, always afforded me the richest lixivium.

Not improved by lime.

Supposing, that the carbonic acid might be an obstacle to the saturation of the potash, I added lime to the mixture, but the lixivium was not improved by it.

I heated red hot, for half an hour, a mixture of 144 grs. of charred blood, with as much carbonate of potash. After lixiviating, 104 grs. were left, 40 having been destroyed.

These 104 grs. were again treated with 144 of carbonate of potash, and were reduced to 62, so that the loss was 42.

First lixivium strongest.

The lixivium of each of these was saturated with the solution of the sulphate of iron of the shops; and the blue produced by the former, after brightening, was double that afforded by the second.

The red heat must be kept up some time.

To ascertain the influence of temperature, I tried three mixtures of equal parts. The first was kept red hot half an hour, the second an hour, the third an hour and a quarter. The lixivium of the first produced but little blue; those of the other two a great deal, and nearly in equal quantities. These results prove, either that the simple prussiate, which predominates in the lixivia, is preserved amid the carbonaceous alkaline mass, or that it is reproduced as fast as it is destroyed.

Calcined blood deliquescent.

Powdered charcoal of blood grows moist in the air. By washing, it affords muriate of soda, and carbonate of soda united with a little prussic acid.

Exhausted by successive lixiviations.

Charcoal of blood lixiviated with potash a second time still affords blue, though but little; a third time, the blue is less perceptible; a fourth time, there is none. This charcoal, thus exhausted, and heated red hot, incinerates with great facility, and without exhaling any smell of ammonia, as that does which is burned immediately after having been exposed to distillation. It seems as if it became more combustible in proportion as it parts with its azote, and approaches

Incinerated.

proaches nearer to vegetable charcoal: nitric acid, however, cannot inflame it. As azote is capable of forming solid combinations able to resist a high temperature, what would be the influence of animal charcoal in the formation of steel? Workmen employ sheep's hoofs for casehardening; has their charcoal any advantage over that of wood?

Would it make superior steel?

Prussiate of ammonia afforded by it.

Equal parts of washed charcoal of blood and potash decarbonated by lime, or lapis infernalis, afforded me by distillation simple prussiate of ammonia, and a great deal of gas, which had the prussic smell, and burnt with a red flame.

Equal parts of the same charcoal and oxide of manganese afforded me carbonate and prussiate of ammonia.

But it does not answer for making sal ammoniac.

The desire of fabricating ammonia with advantage led me to the following experiment. I distilled a mixture of six drachms of charcoal of blood, two drachms of clay, and two of muriate of soda; but the product of sal ammoniac was less than I had expected.

Prussiates from some vegetable charcoals.

All vegetable charcoals azotized are fit for making prussian blue. Thus those of gluten, chich pease, indigo, and pitcoal, afforded me tinging lixivia, sometimes mingled with hidrosulphuret: those of sugar and sugar of milk did not give the slightest indication of blue.

Charcoals of chesnut tree and heath do not contain nitrogen.

The charcoals of the chesnut tree and heath, which smiths prefer, because they have the property of not burning any longer than they are blown, do not derive this from azote, for their lixivia contain no prussic acid.

Cream of tartar and sal ammoniac in certain proportions afford a prussiate.

Cream of tartar heated red hot affords a lixivium, which does not afford the least indication of it: two parts of cream of tartar and one of sal ammoniac, the same: but one part of sal ammoniac, with four of cream of tartar, yields a lixivium, that contains simple prussiate, and affords blue with the green sulphate of iron of the shops. Cream of tartar and nitrate of soda afford nothing.

This proves, that animal charcoal is preferable to vegetable on account of the azote merely. It also follows, that, if we should sometime or other discover an azotized compound more capable of sustaining a strong heat than the ammoniacal salts, we might be able to form prussic acid, perhaps, in a less laborious manner than by means of animal charcoal.

Examination

Examination of the lixivia.

By distillation these constantly give prussic acid and ammonia, the origin of which we have seen above. Lixivia examined.

They also contain carbonate of potash in large quantity; simple prussiate of potash; triple prussiate of potash; sulphate of potash; phosphate of lime; and sulphur.

They let fall the phosphate of lime as they are evaporated; how it was sustained in them I know not.

If a portion of the lixivium be saturated with sulphate of iron, and the liquor with which the blue produced is brightened be examined, phosphate of iron will be discovered in it. It was this phosphate, that led Westrumb to suppose the acid of prussian blue to be the phosphoric.

Alcohol applied to the concentrated lixivia takes from them some simple prussiate; but it appeared to me difficult, to exhaust them of it by its means. The triple prussiate remains in the lixivium with the carbonate.

Of these two prussiates one only can produce prussian blue with the red oxide of iron, which is the triple prussiate, and this because it contains black oxide of iron. The simple cannot, because it is destitute of this black oxide: but it acquires this property, and is converted into triple prussiate, as soon as the lixivium is mingled with the sulphate of iron of the shops; and consequently, if a sulphate completely red be employed, we shall have much less prussian blue, because, the black oxide failing, it cannot form a triple prussiate and afford blue with the same sulphate. Two experiments will render this evident. Common sulphate of iron more advantageous for making prussian blue than the red sulphate.

I divided a lixivium into two equal parts. One was precipitated with red sulphate, the other with the green sulphate of the shops. The surplus oxides being separated by the brightening liquor, the blue from the second was found to be to that from the first in bulk as four to one. Experiment.

The first lixivium, when filtered, had a strong kernel smell. I saturated it with potash, to fix the free prussic acid afresh; and on trying it afterward with the red sulphate, it did not afford an atom of blue, but with the green it yielded a great deal. Hence we may conclude, that a carbonaceous lixivium cannot yield all the blue it is capable of producing with the solution

solution of red oxide, without the assistance of black oxide. Hence the risk of losing all the simple prussiate contained in a lixivium, if we were to use only a sulphate, the oxide of which is completely red; though this I formerly recommended, but it was from mistake. I was not aware, that, if the green sulphate have the inconvenience of affording a pale prussian blue, the oxigen of the atmosphere soon remedies this; and that it has the essential advantage of furnishing the simple prussiate with that portion of black oxide, which is necessary to convert it into a triple salt, and enable it afterward to produce blue with the red oxide. Thus practice had attained the object before theory: but practice in turn becomes a rational process, as soon as theory comes to its justification. Two other experiments will corroborate this.

The blue from the green sulphate is pale at first, but grows deeper by exposure to the air.

Alum employed,

The lixiviums are commouly precipitated by a solution of four parts of alum, and one of the sulphate of iron of the shops.

but it has no chemical effect in the process.

I divided one of these solutions into two parts. One was superoxided by the oxigenized muriatic acid, the other was not. I afterward saturated them with the carbonaceous residuum. The common solution afforded abundance of blue, but the superoxided yielded only a pale precipitate, which was nothing but a little blue diffused among a great deal of alumine. This experiment does not differ at bottom from the preceding: it has only the advantage of showing, that alum is merely a passive ingredient in forming prussian blue.

The lixivium from blood not precisely the same as that from prusssan blue itself.

The lixiviums of the manufacturer, therefore, are not like those made by treating prussian blue with an alkali. The latter will always afford abundance of blue, because it is made a triple salt in the operation itself: but this is not the case with the former; they afford blue only in proportion to the quantity of triple salt they contain, and to augment this, or to convert their simple prussiate into it, it is indispensable to employ a sulphate, that, if not strictly green, at least is so in a certain degree; and this is precisely the case with the sulphate of the shops, however long it has been made.

In the calcination of the blood probably

From these details we learn farther, that, if the lixiviums contain but a certain portion of triple prussiate, it is either because

because the charcoal of blood has not iron sufficient to convert all the simple prussiate that is formed during the calcination into a triple salt, or that part of this salt is again reduced to the state of simple prussiate by the loss of part of its oxide, as we have seen takes place when it is heated alone. Of these two opinions, however, I should be inclined to adopt the latter, because I have observed, that the ashes of the charcoal that has been lixiviated always afford a great deal of iron: we have no reason, therefore, to suppose, that in calcining the carbonaceous alkaline mixture iron is wanting to the prussiate; and indeed, if we reflect on the subject, it is surprising, that the triple prussiate, which actually exists in the lixiviums, should have been capable of defending its oxide against the effects of the carbon continually tending to reduce it. The whole of this, however, is at present very obscure: we neither know the period when the prussic acid is formed, whether it be destroyed to be again reproduced, nor, lastly, the degree of heat required to obtain the greatest possible quantity of either of the prussiates, that are the objects of the manufacturer.

part of the triple prussiate destroyed.

The existence of the triple prussiate in the lixivia may be demonstrated by the following experiment.

Proof that the lixivia contain triple prussiate

Saturate a lixivium with aqueous sulphuric acid. The carbonic acid first flies off, and next the prussic acid of the free prussiate. After this, heat must be applied, when the triple prussiate is attacked, and the white prussiate of iron is made apparent. Beside this, concentrated lixivia long kept deposit octaedral crystals of triple prussiate.

The prussic lixivium has two very distinct tastes; one of potash, the other of kernels. By the latter we may judge at once of its quality. If it affect the palate but slightly, the lixivium is defective, either because the mixture was insufficiently heated, or the animal charcoal in too sparing proportion. I am of opinion too, that the calcination of the mixture in the open air does not contribute to the augmentation of the prussiate; and that it would probably be more advantageous, and save trouble, to heat it in covered crucibles in a reverberatory furnace, since it has been proved, that stirring the mixture is not necessary to the success of the operation.

Their quality may be judged by the kernel taste.

When

Previous to evaporation a little green sulphat should be added, to prevent the loss of the simple prussiate.

When it is necessary to concentrate the lixivium, either to keep it, or that it may take up less room, we should previously take care, as Curaudeau perceived, to prevent the simple prussiate from being destroyed. This is readily done by adding green sulphate in small quantities. In this way it dissolves completely in the lixivium, which first grows red, then becomes again yellow. An excess of sulphate does not alter it, because the potash, which predominates, reduces it to an oxide; and this falls down, without being able to pass to the state of a prussiate. To attain this, it must present itself accompanied by an acid; for the oxide here spoken of is entirely that at a minimum, which has no action on the triple prussiate.

Advantage of this.

I have divided a lixivium into two equal parts: one was prepared, or converted into a triple salt, by the green sulphate; the other was not. I afterward distilled them: and the first gave no indication of ammonia; the second furnished it as usual. It is indispensably necessary therefore, to prepare the lixivia before they are concentrated. Finally, neither the red oxide, nor its sulphate, as Scheele found, is capable of dissolving in the simple prussiate, and giving it the properties of the triple. This oxide too, though adapted to become the basis of prussian blue, is equally unable of itself, to decompose the triple prussiate; it must be presented to it in solution by an acid.

The red oxide does not unite with the simple prussiate,

or decompose the triple prussiate alone.

Recapitulation.

Component parts of prussic acid.

Prussic acid is composed of carbon, nitrogen, and hidrogen, in proportions of which we are yet ignorant. We can only conjecture, from the great quantity of carbon it leaves in several instances after it is destroyed, that this principle enters into its basis in a very large proportion, compared with the others. Neither is there any fact, that indicates oxigen to make a part of it; and indeed, from the well known affinities of its three elements, added to the circumstances under which the acid is formed, we can scarcely think it does.

No oxigen.

Has few acid qualities.

The prussic acid in its separate state has very few of the common qualities of acids. It has not a sour taste; it does not

not redden litmus; it does not dissolve in water, the proper menstruum of acids, so well as in alcohol; and it is even decomposed in it spontaneously without access of air. With alkalis it forms combinations so imperfect, that we find in them the specific properties of their component parts scarcely altered; and the weakest of all acids, the carbonic, is capable of decomposing them. In short, its combustibility, taste, smell, generation amid volatile oils and kernels, and quality of keeping in alcohol, form an assemblage of properties, by which it approaches much nearer to oily and inflammable productions than to saline substances.

More analogous to oils and inflammables.

The prussic acid, however, notwithstanding its little saline energy, attacks the oxide of mercury at a maximum with great advantage; and with this oxide it furnishes a saline combination so strongly characterized in its qualities, that we are obliged to acknowledge it acts under certain circumstances as one of the most powerful of acids. In fact, the prussiate of mercury wants nothing to entitle it to rank with the most perfect metallic salts; and what may perhaps be deemed astonishing is, to see it refuse to combine with the oxide at a minimum; yet, from the effect of concurrent affinities, of which there are other examples, it raises it to the state of an oxide at a maximum, separating one portion of the metal, to form a prussiate with the other.

Yet toward oxide of mercury it acts as a powerful acid.

Pussiate of mercury.

Prussic acid has no action on the red oxide of iron; but it very readily attacks the black oxide, and produces with it white prussiate. It is true this prussiate is not strictly white, on account of the difficulty of preparing a precipitate with the green sulphate totally free from a surplus of oxide; and accordingly it is always greenish: but as it becomes a perfect prussian blue by drying, there can be no doubt, that the prussic acid and the base of green sulphate, if not affected by contingent circumstances, would furnish a prussiate as white as that which we obtain in a more easy manner.

Action of prussic acid on iron.

Prussian blue is not a single compound, as had been supposed. This the following observation is sufficient to show. It is known, that the base of this blue is the red oxide of iron: but, if this oxide alone were sufficient to make prussian blue, why is not this blue afforded by prussic acid and red oxide? and why do not the alkaline prussiates produce it with

Prusisan blue not a single compound.

with solutions of this oxide? Something else therefore is wanting to the prussian blue, and the following facts will complete the proof.

Proofs of this.

On applying potash to prussian blue we obtain a yellow chrystallizable salt, which has always a constant proportion of black oxide of iron. If we employ this yellow prussiate to reproduce prussian blue, the oxide repasses into the new combination with the prussic acid. The black oxide then is a necessary element in the formation both of the crystallizable prussiate and of prussian blue; as well as of all the metallic prussiates, that are prepared with the triple prussiate of potash.

Prussiates formed by different metals.

There are some metals, that are capable of forming both simple and triple prussiates, as copper, silver, manganese, cobalt, nickel, uranium, &c. There are others, that form only a simple prussiate, as gold, mercury, &c. There are some, that admit only of a triple prussiate, as iron, &c. And lastly others appear to be incapable of combining with the prussic acid. Except prussian blue, however, and the prussiate of mercury, we know but little of them, and they deserve further examination.

Union of black oxide of iron with the prussic acid.

The black oxide of iron combined with prussic acid can pass from one combination to another without changing its state. The base of this combination may even be raised from a minimum to a maximum, without the black oxide participating in the change. The combination of the acid with this oxide is bound by an affinity so powerful, that the alkaline hidrosulphurets cannot separate them; or attack the oxide, if you please, either in the triple prussiate, or in the prussian blue.

The prussic acid, combined with that portion of black oxide which enables it to form triple prussiates, either alkaline or metallic, is a peculiar combination, the existence of which is not doubtful, but of which we know nothing separately from these prussiates.

Heat reduces the triple prussiate of potash to simple,

The triple prussiate of potash cannot support a red heat, without losing the black oxide, and consequently being reduced to a simple prussiate.

and decomposes the simple prussiate.

The simple prussiate is decomposed likewise, but by a far lower temperature: its acid is destroyed, and reduced to ammonia and carbonic acid: and it is the destruction of this salt

salt by the heat of ebullition, that injures the lixivia for preparing prussian blue.

Simple prussiate converted into triple.

The simple prussiate assumes the character of triple prussiate, as soon as black oxide of iron, or a salt with this oxide for its basis, is presented to it; and thus acquires, beside the advantage of being crystallizable, that of not being decomposable at a boiling heat.

The test liquor.

This prussiate, which was the test liquor so much sought after by chemists, does not afford prussian blue with solutions of red oxide of iron; but it produces this blue if they contain any black oxide, because its acid immediately attaches itself to that portion of the black oxide, which will serve as an intermedium between it and the red oxide.

Decomposition of the triple prussiate of iron, & of mercury.

The triple prussiate of iron, or prussian blue, strongly heated, is reduced to ammonia, carbonic acid gas, gaseous oxide of carbon, steeled iron, and carbon.

The prussiate of mercury affords the same products by its decomposition, and likewise a certain portion of oil.

The lixivia.

The lixivia of the carbonized materials contain little triple prussiate, but a great deal of the simple; and they muft not be boiled down, till the constitution of the second is strengthened by an addition of black oxide, or of green sulphate.

How to obtain the most blue from them.

To obtain from these lixivia all the prussian blue they are capable of affording, it is indispensably requisite, to employ a sulphate of which a portion at least is green; without which the simple prussiate they contain cannot furnish blue with a sulphate of which the base is completely red.

To conclude, if the reader take the trouble to compare this paper with Scheele, he will find, that all the truths it contains were perfectly known to him; but I conceived they required to be more fully explained, which I have here attempted.

III.

An Account of a Stove for Heating Rooms, or Drying different Articles; by Mr. G. Field, *of Newman-Street.**

THE various advantages of heating, boiling, steaming, evaporating, drying, ventilating, &c., are united in this stove; so

* Transactions of the Society of Arts, for 1806.

so that it is capable of being applied to many useful purposes, both in domestic economy and the arts: on which account, a silver medal was voted by the Society of Arts to the inventor. The subjoined description, with the annexed plate, will more fully explain its design.

Description of the stove.

Fig. 1. Pl. VII. Represents a longitudinal section of the stove, showing the course of the air from its entrance into the flues of the stove at A, to its entrance into the upper chamber of the stove at B: and also, the course of the smoke from the fire-place at C, till it escapes from the stove at D. E, E, are the doors or openings of the fire-place and ash-hole.

Fig. 2. Is a similar section at right angles with the above, exhibiting the course of the air through the chambers of the stove, from its entrance into the chamber No. 1, at B to its entrance beneath the fire-place at F. This figure also shows sections of the flues, with the divisions through which the air and smoke pass separately, the smoke flue in the centre, and the air flues on each side. G, G, are doors and openings through which the articles to be dried are introduced into the chambers.

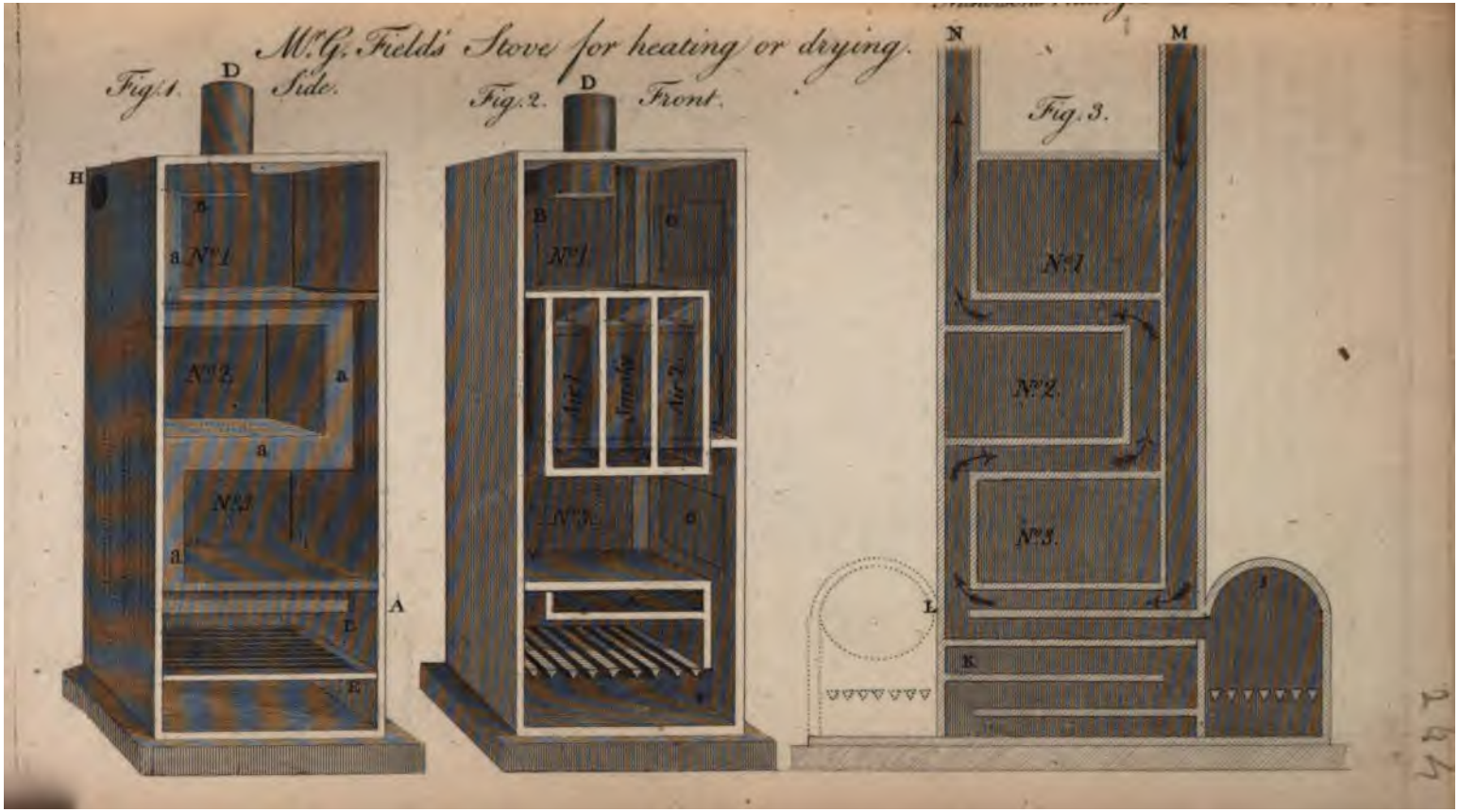
When the fire is lighted, and the doors of the chambers, ash-hole, and fire-place, closed, the air by which the fire is supplied enters at A, Fig. 1, passes through the air-flues *a*, *a*, *a*, *a*, enters the upper chamber at B, traverses and descends through the chambers No. 1, 2, 3, and arrives beneath the fire at F, Fig. 2. Having supplied the fire with oxigen, it passes through the flue with the smoke, and escapes at D, heating in its protracted course the chambers and air-flues.

Its use as a drying stove,

As the cold air enters the stove at A, immediately above a plate forming the top of the fire-place, and pursues a similar route with the fire-flue, it enters the chambers very much heated and rarefied. Hence any moist substance placed in the chambers evaporates, in consequence not only of the heated flues circulating round them, but of a stream of warm rarefied air, which, while it continually causes evaporation, as continually bears away the exhaled moisture in its passage to the fire, thus imitating the gradual and efficacious plan of nature in drying by the sun and air. While these effects are taking place within the stove, part of the air which enters at A, Fig. 1 and 2, passes through air-flues on the other side

and in warming rooms.

of


M.r G. Field's Stove for heating or drying.
Fig. 1. Side.
Fig. 2. Front.
Fig. 3.
D
H
a
N.o 1
N.o 2.
N.o 3
A
E
B
c
Air 1
Smoke
Air 2
N
M
L
K
J

with the great fires they employ for their boilers. It has been shown to be useful in the confectioners art, and probably it may be equally so in baking biscuits for the navy; nor less so in drying linen for the laundress, dyer, calico-printer, and bleacher. I have myself found it well accommodated for a chemical elaboratory.

The efficacy of the stove in ventilating, boiling, and steaming may easily be shown. In manufactories and rooms generally the heated and noxious part of the atmosphere ascends towards the ceiling: if then the air-flue M, Fig. 3, is continued upward according to the height of the room in which it is placed, the air will be drawn from the top, and the room become ventilated, while from the opening at N it is supplied, if requisite, with warm air.

It is unnecessary to show the various ways in which a boiler may be connected with this plan: it is sufficient to observe, that in the space allotted for the fire-place in Fig. 1, there is sufficient room within the body of the stove for this purpose; and that if the circulating air be made to pass over the boiler, evaporation may be carried on very expeditiously by the air removing the vapour as it arises. Finally, if another division of the flues be made in the manner shown Fig, 2, it might form a steam-pipe or flue, running the course of the air and fire-flues, to convey steam to one or more apartments of the stove; or extended beyond the stove for heating the room in which it stands. One of the air-flues might occasionally be adapted to this use. It is obvious that the power of steam in a heated apartment would be not only greater, but better kept up. In steaming it would be necessary to close the apartments of the stove, and to give air to the fuel by a different course.

Its form and dimensions admit of considerable variations.

As the stove is not confined in its dimensions, so neither is it necessarily of the form described in the drawing, nor are the apartments necessarily three: all these particulars admit of variation according to the local or other circumstances. It is evident that the air-flues themselves may be converted into chambers for drying, &c.; and the fire-place of Fig. 3 is well adapted to receive an apparatus for the decomposition of coal, &c.; for producing all the effects of the thermo-lamp, or illuminated smoke, &c. But it is needless to enumerate

merate the many economical and philosophical uses to which the stove may be applied. It is sufficient for the present purpose, if I have rendered the principle and plan intelligible, the artist and manufacturer will then be at no loss in adapting it to the particular object, which he may require to accomplish.

A certificate from Mr. S. Sellers, Chemist, Broad street, Bloomsbury, stated, that the effects of the stove in question are as Mr. Field has described them in his paper.

IV.

Inquiries concerning the Oxidations of Iron; by Mr. DAVY.

(Continued from page 226.)

Oxides by Solution.

THE solutions of iron afforded me still more satisfactory results, both because they confirm and render more clear those obtained by calcination, and because they may throw much light on a number of manufactories, and simplify the chemical theory of iron. But, convinced as I am of these results, I offer them only as conjectures, since they are diametrically opposite to the present mode in which solutions of iron are viewed, and I am always afraid of being led into errour. Solutions of iron.

The inconstancy of the green oxide, which Lavoisier and Mr. Proust obtained when 100 grains of iron had taken up 37 of oxigen; and which, from the experiments I have related, varies from a few hundredths to 32; could not but lead me to conclude, that the properties distinguishing this oxide from the red, are owing not to a fixed degree of oxigenation, but rather to a certain density, which allows the water, or the acid, or both, to lodge in the interstices of each molecule; and hence the difference of colours of these precipitates by alkalis, prussiates, and gallates, and their less or greater solubility; the only properties that distinguish the green salts from the red. Green oxide variable. Why?

White oxide.

This reasoning led me to examine not only the red and green oxides, but at the same time the white oxide lately announced by Mr. Thenard*, of which I had beforehand some doubts†. I am sorry to call in question the labours of men whom I greatly esteem and respect, but I conceive I am seconding their views, if my observations be just.

Of the White Oxide.

Its characters.

The characteristics assigned to this oxide by Mr. Thenard are, 1st. that it becomes green by exposure to the air; 2d. that it is changed to green or yellow by oxigenized muriatic acid; 3d. that when the precipitate is made in a phial, and care is taken to stop it close, an absorption is observed on shaking it to convert the white oxide to green; which proves, that part of the oxigen of the air in the phial combines with the white oxide, and changes its colour. I shall take the liberty of examining these facts.

Change of colour in precipitates no proof of a different dose of oxigen.

From the experiments of Messrs. Fourcroy, Proust, and Berthollet, Mr. Thenard, and all the chemists of the present day, are of opinion, that the change of colour in precipitates does not in general indicate a different degree of oxigenation. The white is that which very frequently conceals the true colour of oxides in almost all metals, that are said to be susceptible of oxigenation, as tin, mercury, copper, silver, lead, bismuth, and probably manganese. This depends on the quantity of acid the precipitates retain,

* See our Journal, Vol. XIV, p. 224.

† Before undertaking my present inquiry I could not avoid doubting, that a few hundredth parts of oxigen could modify the colour of metallic oxides so far as to change them from white to green, black, &c. All the facts with which I am acquainted militate against this property of oxigen; consequently, I was suspicious of every precipitate, the colour of which varied much from that of the oxides of the same metal obtained by calcination. If white, I argued, be the result of the combination of iron with a few hundredths of oxigen, why does not this colour present itself in the course of the calcination of iron? and why in the calcination of manganese, copper, and bismuth, do we not perceive the same phenomenon? Besides, the only well established white oxides are those of antimony, zin, and arsenic; and from the moment these become white they never change their colour, even from a considerable over dose of oxigen.

they

they being freed from this with more or less facility, according to their nature, and still more according to the circumstances under which the precipitation has taken place. This appears to be the case with the white precipitate of iron.

Conditions requisite to precipitate iron white.

The conditions requisite to throw down a white precipitate from a green salt of iron, are: first, that the solution be highly concentrated; second, that the precipitating alkali be in some degree the same. This did not escape the sagacity of Mr. Thenard, who recommends boiling dilute sulphuric acid on an excess of iron filings, that the formation of the white oxide may be more sure to succeed. In fact, whenever a tolerably concentrated solution of alkali is added to such a solution of iron, the alkali first seizes a portion of the acid, and probably a little of the water, and precipitates a white sulphate of iron, which frequently crystallizes, though irregularly, at the moment of precipitation, and still retains a sufficient quantity of acid, to be soluble in water. It likewise constantly turns sirup of violets green, and forms a red precipitate in water coloured with litmus, as is common to salts of iron with excess of oxide*.

Action on sirup of violets and litmus.

Proofs of the fact.

It is easy to verify this fact in a convincing manner, by letting fall a few drops of such a solution into an excess of alkali. If, after having left the white precipitate in it eight or ten minutes, the fluid be decanted, or if it be drawn off

* I have obtained several salts of iron, which immediately produce a red precipitate with infusion of litmus, and at the same time turn sirup of violets green. The property of precipitating infusion of litmus always indicates a salt that is at least neutral, and must not be confounded with the effect of simply changing its colour. In the mere change of colour the small portion of alkali in the infusion combines with an excess of acid, and quits the colouring matter, which it modified. There can be no doubt, that a salt exhibiting this phenomenon has an excess of acid, since there is sufficient to saturate the alkali in the infusion, without occasioning a precipitate; and on this principle is founded the use of this reagent. When the change of colour is accompanied with a precipitate, the alkali, not finding an excess of acid to combine with, seizes part of that which held the oxide in solution, and the precipitated oxide carries down part of the colouring matter with it. What appears extraordinary is, that oxide of iron does not turn sirup of violets green, though neutral salts of iron, or salts with an excess of oxide, have this effect.

Infusion of litmus gives a precipitate in certain cases.

Anomaly of oxide of iron.

by

by a siphon, which is the surer way; and a few drops of water be afterward added, to take up the alkali, that adheres to the surface of the precipitate and the sides of the glass; a mass of sulphate of iron will remain, great part of which will dissolve in water, and exhibit all the properties I have just mentioned. I have left this precipitate for twenty-four hours macerating in potash, and at the expiration of that time have still obtained soluble sulphate of iron.

Ammonia shows this more strikingly.

In a concentrated solution of ammonia, the result is still more striking; because, as the density of the precipitated sulphate of iron greatly exceeds that of the ammonia, the precipitate falls quite to the bottom of the glass, and great part of it sticking to this, escapes the subsequent action of the alkali; which does not happen with the concentrated fixed alkalis, the density of which is greater than that of the ammonia, and in consequence they envelope it on all sides.

Why a green precipitate is thrown down.

This is the reason why these solutions, which when concentrated throw down a white precipitate, throw down a green precipitate when they are diluted with water freed from air by long boiling. For this reason, limewater never gives a white precipitate, even with the most concentrated solutions. Lastly, for this reason I suspected before hand, that the muriates and nitrates, which give white precipitates with the alkalis, would be precipitated green by barytes and strontian water, which in fact is always the case. The green colour cannot be attributed to a superoxidation occasioned by the air in the lime, barytes, or strontian water; for, beside that the rapidity of the operation, and the quantity of air that so small a bulk of distilled water could contain, would not justify such an idea, I took the precaution to boil the water in which I dissolved the three earths for an hour and half.

Not from additional oxigen.

The same if diluted even with sulphuretted hidrogen.

If, instead of diluting the solution with water, it be diluted with sulphuretted hidrogen, in which the presence of oxigen cannot be suspected, the result will be still the same; the precipitates formed by alkalis will be green or black, and never white. It is true Mr. Thenard says, that, on pouring sulphuretted hidrogen on a red solution of iron, this will yield

yield green or white precipitates. I am inclined to think, however, that this is rather an inference drawn by Mr. Thenard from his manner of considering the white oxide of iron, than a fact that came under his observation. Be this as it may, I can assert, that, having repeated these experiments several times, frequently changing the reagents, and varying the circumstances as much as possible, I have obtained only green or black precipitates, according to the concentration of the sulphuretted hidrogen, and the quantity of the sulphate of iron, presented to each other. If this assertion of Mr. Thenard were a fact he observed, I confess it is an anomaly, for which I cannot account, and which I cannot reconcile with the whole of the facts I have related.

The existence of a green oxide questionable.

If all these facts do not controvert the existence of the white oxide, I have still another to add, which not only controverts it, but even renders that of the green oxide questionable.

Let a phial be filled with three parts of ammonia, and one of sulphate of iron precipitating white; and let it be corked immediately. At first, a white precipitate will be formed, which on shaking the phial will dissolve in the ammonia. If the oxide of iron be afterward thrown down by means of water or of an acid, it will always be green or brown: and if, instead of precipitating it by either of these means, a small curved tube be fitted to the phial, its extremity be immersed in water, and sufficient heat be applied to expel the ammonia; as this is volatilized, a black or brownish oxide will fall down, which, on being dissolved in muriatic acid, will afford precipitates for the most part red. Now it is impossible here to suspect a superoxidation by the ammonia.

White salts of iron with base of red oxide.

Add to this, there are likewise salts of red oxide of iron with excess of oxide, that are not only white and soluble like the salt of Mr. Thenard, but frequently crystallize; that are not deliquescent, like the common red salt of iron; and that exhibit still other peculiarities. I shall mention these in a paper on another subject, where I shall speak of iron incidentally. Till that occasion, likewise, I shall defer explaining the cause, why some of the white precipitates observed by Mr. Thenard retain their colour even after long boiling.

boiling. The fact is very true, but oxigen has nothing to do with it.

Effects of oxigenized muriatic acid.

The changes occasioned, according to Mr. Thenard, by pouring oxigenized muriatic acid into a sulphate of iron precipitating white, agree very well with the idea I have formed of this precipitate. As oxigenized muriatic acid is so little soluble in water, and the sulphate of iron in question so concentrated, it follows, that, if the acid be not very abundant in the solution, the precipitate will be green; because the little oxigen contained in the muriatic acid is capable of converting to red oxide but a small portion of the green, which is so predominant as to envelope the red, and prevent its appearance. It will be the same to the eye as if the solution were diluted with water, equal in quantity to the muriatic acid, by which the white precipitate would have been changed to green in a similar manner. If, on the contrary, the oxigenized muriatic acid be very abundant, there is no doubt, that the solutions will become red, as is the case with all the green salts of iron.

On precipitating in a closed phial, air is not absorbed but evolved.

With respect to the third fact, namely, that on making the precipitate in a phial, and stopping it immediately, absorption takes place, and the residual air extinguishes a candle, I shall only say, that instead of absorption, I have always found an evolution of air, which has sometimes forced out the cork. It is true, that, after the white precipitate has passed to green or red, the residual air sometimes extinguishes a candle; but this is owing to the extrication of a principle, of which I shall speak presently.

Of the green oxide.

Green oxide offers three questions to be solved.

The green oxide obtained by dissolving iron in acids, offered me three leading facts to examine. First, to determine how much oxigen iron takes to pass to the state of green oxide: secondly, to account for its colour: thirdly, to observe the influence of atmospheric air on these solutions.

Proportion of oxigen.

To determine the proportion of oxigen, and observe at the same time the influence of the atmospheric air, I took 90 grains of iron filings, which I divided into three equal parts. Each of these was dissolved separately in muriatic acid diluted with

with water. As soon as the solution was complete, I precipitated one portion of 30 grains by ammonia, washed and drained it as quickly as possible, and dried it at a temperature of about 120° [300° F.]. After it was dry I found it a brown oxide, attracted by the magnet, weighing 36½ grains, and precipitated red from its solution in muriatic acid. Another portion I precipitated likewise by ammonia; but with a view to obtain the precipitate red, I diluted the solution with five or six parts of water at 50° [144°] before I added the ammonia. This oxide was in fact red; and when dried like the preceding, it was not at all affected by the magnet, though it weighed only 36 grains. Lastly, I precipitated the other 30 grains by ammonia likewise, using a very broad vessel, in which I left the precipitate exposed to the air for a month, stirring it twice a day. At the end of this time I dried it like the preceding: it was red, gave no signs of magnetism, and weighed 36·2. The only difference between all these oxides was, that the first was brown and magnetic, while the others were red, and did not become magnetic till exposed to a higher temperature.

Brown magnetic oxide.

Red, and not magnetic.

The same.

Yet all equally oxided.

Though I perceived in the course of this experiment, that it was not sufficient to establish with accuracy the proportion of oxigen, that the green oxide contains in solutions of iron by acids, on account of the oxigen that must combine with it during its being dried at so high a temperature, and in a state of such minute division, it confirms two of the principal results obtained in the oxides by calcination. This process afforded me red oxides that had only ·15 or ·20 of oxigen and the solution produced red oxides that contained only ·20 of oxigen, including what was absorbed during the drying. Calcination afforded red magnetic oxides; and solution did the same.

This does not ascertain the proportion of oxigen absorbed during solution.

There are two methods of appreciating with extreme accuracy the quantity of oxigen contained in the green oxide by solution. The first, which I should have preferred, if circumstances had permitted me to adopt it, is, to dissolve a given quantity of iron in muriatic acid, and carefully to collect the hidrogen evolved; this measured, and for still farther certainty burnt in Volta's eudiometer, would give the quantity of oxigen combined with the iron. The second is

Two methods of doing this.

to

to dissolve a given quantity of iron in muriatic acid, and, after having precipitated it by an alkali, to dry it in an exhausted receiver by means of a lens.

Influence of the air on solutions of iron.

Oxigenation not the cause of the difference of the salts.

All the false notions diffused through the pneumatic theory of iron arise from having ascribed the colour and other properties, that distinguish the green and red salts, to a difference of oxigenation. This difference once established as a principle, nothing was more natural, than to refer to the same cause the transition of the salts of iron from green to red, on exposure to the air; particularly as the circumstances, that sometimes accompany this phenomenon, readily agree with this explanation.

Mistake of Scheele.

The authority of Scheele gave additional weight to this illusion. This celebrated chemist had observed, that, on dissolving green sulphate of iron in water, a sediment of green oxide commonly remained; and hence he inferred, that it was owing to the air contained in the water, which superoxided part of the green oxide; and this, becoming red, increased its saturating power with respect to the acid, and precipitated it. He then gave this process as a mean of ascertaining the quantity of air contained in water. Great as is the authority of this illustrious chemist, I must take the liberty of observing, that, even were this phenomenon owing to a superoxidation by means of the air contained in the water, this mode of appreciating the quantity would not be exact; because the quantity of precipitate does not depend solely on the red oxide formed by the air, but rather on the degree of acidity of the sulphate, which must always be supposed uniform according to Scheele's theory, but this is contradicted by experience. Thus admitting the superoxiding action of the air, a quart of water poured on a pound of very acid green sulphate would let fall no precipitate; while a quart of the same water would throw down a considerable quantity from another sulphate little or not at all acid.

The air in the water has no effect.

The explanation of this phenomenon too is defective in itself, for it takes place equally whether the water contain air, or be perfectly free from it. I made this comparative experi-

experiment with two equal quantities of distilled water, from one of which the air was completely expelled, while the other was saturated with it artificially, and the results were the same with both. If the crystals of sulphate I used were white, no deposition took place; but if they were green, a precipitate fell down, which was equally bulky in both solutions: so that Scheele's process is calculated rather to show the acidity of the green salts of iron to a certain point, than the quantity of air in the water.

The quantity of oxigen gas in the water must be far too small to produce the effect.

It may be objected to me, that, from the experiments of D^r. Carradori, boiled water always retains a little air: but, beside that I have lately repeated the experiment with water freed from air by Dr. Carradori's method with the same success, the experiments of Henry*, Humboldt, Gay Lussac, and more especially of Dalton†, on the absorption of gasses by water, completely terminate the controversy. According to Dalton, to whom I refer because he more directly turned his attention to this point, water saturated with atmospheric air contains only 2·012 per cent of its bulk, of which ·778 are oxigen gas, and 1·234 nitrogen. Consequently 100 cubic inches of water contain about $\frac{1}{4}$ of an inch of oxigen. Now if we consider, that the greater part of this gas is expelled by boiling; and if besides we make a correction for the heterogeneous substances, which according to Lambert and Saussure the air always contains, we shall find the influence of the oxigen contained in the water, even supposing it saturated with it, must be nothing; for if a green salt of iron with excess of oxide be thrown into 100 cubic inches of water at 60° [167° F.], a precipitate of at least 15 or 20 grains of red oxide will be formed, which cannot be attributed to the oxigen of the air contained in the water, unless the experiments of the learned natural philosophers I have quoted be altogether futile.

Proofs that the air does not oxigenate solutions of iron

Beside this experiment; beside the ammoniacal solution of green sulphate, which passes to red without the possibility of suspecting the presence of oxigen; beside the precipitate, which did not increase its oxigenation 1 per cent by

* Philosophical Transactions for 1803, or our Journal, Vol V. p. 229.

† Manchester Mem. N. S. Vol. I, or Journal, Vol. XIII. p. 291.

exposure

exposure to the air for a month; I have made several other experiments tending to the same object: and they have all convinced me, that the air has no superoxiding action on solutions of iron, at least at the common temperatures. Of these I shall recite only two, that are among the most conclusive.

Farther proofs.

1. I dissolved two equal portions of iron under circumstances perfectly similar. One of the solutions I put into a glass three inches in diameter, and immersed in it a curved tube, the extremity of which was a ball pierced with several holes. Through these I passed atmospheric air for seven hours at different times. At the expiration of three days, I compared these two solutions in various ways, and found, that the one into which I had forced air was perfectly similar to the other, which was not perceptibly altered, though the temperature was 12° [59° F.]. 2. By means of the same apparatus I passed about three quarts of oxigen gas through a solution of ten grains of iron, and, though the temperature was 25° [88°¼ F.], it had no action on the solution.

On the colour of the green oxide.

Particles of precipitated oxide of iron hollow spheres containing a fluid.

On adding a few drops of alkali to a solution of iron a little diluted, I observed, that every particle of the oxide was formed of a very thin pellicle, including some fluid, and I accounted for the green colour by the difference of density between this pellicle and the fluid it enclosed. I likewise ascribed the alteration in green solutions exposed for some days to a temperature of 20° [77° F.] to the bursting of these vesicles by the dilatation of the fluid contained in them. To the pressure exerted upon these vesicles I attributed the unchangeableness of these solutions in bottles quite full and close stopped. But I could not reconcile with these modes of viewing the subject the change, that is induced in green solutions of iron by oxigenized muriatic acid, and in red solutions by sulphuretted hidrogen. The nature of the constituent principles of these two reagents renders the manner in which they are supposed to act in these two experiments so plausible, that I should not have withheld my assent to them, had I not been convinced by all the facts I have related,

This might account for their change of colour in some cases,

but not in others.

related, that oxigen has no influence in the red or green colour of oxides of iron. I meditated some experiments therefore, tending to observe the mode of action of these two reagents, when I recollected a fact observed at the beginning of my inquiry, which thus indemnified me for a number of errours, into which it had led me before.

White precipitate from iron.

In every kind of iron I had hitherto treated I found a substance, that fell down in a white precipitate, did not change by exposure to the air, gave an emerald green precipitate with prussiate of potash, and which I believe to be what Bergman called siderite, rather than phosphate of iron. On the other hand I obtained from some red salts of iron a white precipitate, that sometimes crystallized in laminæ very soft to the touch, which the most experienced mineralogist would take for French chalk, but which is nothing but a salt of iron with excess of oxide. I thought at the time, that these two substances were the same, that they were magnesia, and that this was nothing but iron at a maximum of oxidation. The name of this earth favoured the illusion; as did the opinion of former chemists respecting the transmutation of metals into earths. I deferred the investigation of this subject to a future period, making in the mean time only a few experiments on magnesia; and I found, that, on treating solutions of this earth with sulphuretted hidrogen, it afforded a precipitate similar to the green oxide of iron.

Supposed to be phosphuret of iron.

Sometimes resembling steatites.

Both imagined to be magnesia.

Sulphuretted hidrogen precipitates magnesia green,

Though my farther reserches concerning iron taught me, that neither of these substances was magnesia, the way in which this earth was coloured by sulphuretted hidrogen was a fact, the reason of which I intended to make the subject of future inquiry; and which, on recurring afresh to my memory, led me to suspect, that sulphuretted hidrogen might have a mode of action different from any with which we were acquainted. I was eager to repeat this experiment, not only on magnesia, but on lime and alumine likewise, and I found in fact, that the soluble salts of these three earths, treated by sulphuretted hidrogen, gave precipitates altogether similar to the green oxide of iron*. These precipitates exposed to

as it does lime and alumine.

* Certain management is necessary, to succeed in this experiment, which I have not repeated often enough, to be able to give certain instruction

The green colour goes off by exposure to the air.

to the air resume their white colour after some time, if they be not shaken; but agitation greatly accelerates this change, which is another similarity between these precipitates and those produced from the red salts of iron by sulphuretted hidrogen.

These green precipitates are hidrurets.

These green and earthy precipitates are not hidrosulphurets, as might be supposed; but hidrurets, which probably retain a little acid. This is proved by their being decomposed by oxigenized muriatic acid, without leaving any traces of sulphur; and by retaining their green colour, and other properties annexed to it, when redissolved in acids; which could not be the case if they were hidrosulphurets, since acids decompose these instantly.

The green oxides of iron are the same.

From these elucidations the cause of the green colour of oxide of iron, and its colouration by muriatic acid, suggested itself as it were spontaneously. The green oxide is never formed, unless hidrogen be set free: a part of this therefore remains engaged in the oxide, and imparts to it the green colour, with the property of being less soluble in water, or more crystallizable. This property of rendering a salt more crystallizable, possessed by a principle of so little density, appears at first sight inconsistent; but it is confirmed by the superoxigenized muriate of potash, which is rendered two or three times less soluble than the simple muriate by the addition of oxigen.

Oxigenized muriatic acid takes away their hidrogen.

Oxigenized muriatic acid then acts on a green salt of iron, as it does on sulphuretted hidrogen, phosphorus, &c. It deprives the oxide of the hidrogen with which it is combined, as it does the sulphur and the phosphorus: which at the same time proves, that in the oxide of iron it is not in the same state of dilatation as that in which we are acquainted with it uncombined, for in this state it does not combine with oxigenized muriatic acid at the temperature of the atmosphere.

The presence of hidrogen in the green ox-

The hidrogen likewise betrays itself in the offensive smell given out by a concentrated solution of iron, when a fixed

structions respecting it. I have left sulphuretted hidrogen in contact with sulphate of magnesia for an hour before it precipitated. Sometimes, the precipitate is green in the very act of falling down; at other times it does not become green till some moments after.

alkali

alkali* is added to it, and the glass shaken a little: and we cannot allege, that this smell is owing to a few globules of hidrogen, that have remained mechanically entangled in the solution, since the same phenomenon takes place, if the solution be previously boiled. If the solution be diluted with six or eight parts of water at 50° or 60° [144° or 167° F.], and it be stirred with a glass rod as the alkali is put in, the smell is still very strong, and continues to exhale as long as an atom of green oxide remains in the precipitate; so that it is easy to tell by this, without seeing the precipitate, whether the solution be red or green. When a little green sulphate of iron in a very concentrated solution, like that which gives a white precipitate, is precipitated in a phial, and this is corked and shaken, it will be seen, that the volume of gas is increased; for, if the cork do not fit very tight, it will be forced out, notwithstanding the temperature continues the same. If the air in the phial be afterward examined, it will be found sometimes to extinguish a candle, or to detonate on its application. Now both these are compatible with the presence of hidrogen, according as it is pure or mixed; and possibly there may be a little iron dissolved in it, as zinc or arsenic sometimes is.

ides of iron proved by the smell,

and by the gas evolved.

To satisfy myself still farther of the presence of hidrogen, and its influence on the salts of iron, I adapted to a tubulated retort a small receiver, and to this a curved tube, the extremity of which opened under a jar in the pneumatic apparatus. Into the retort I poured a solution of green sulphate of iron recently made, having previously boiled it half an hour, to prevent any suspicion of hidrogen mechanically retained in it. This solution I precipitated with caustic soda greatly diluted with boiling water. As soon as the mixture was brought to boil, a gas fetid as hidrogen was evolved, which detonated on the contact of flame. The water of the pneumatic apparatus too had the nauseous taste and smell of hidrogen disengaged from solutions of iron.

Farther proof by expelling the hidrogen.

In order to expel all the hidrogen, or to convert all the oxide from green to red, I continued the distillation.

* If ammonia be used, its smell conceals that of the hidrogen.

Scarcely

Scarcely was the mass dry, when the retort burst, and I found in it more than 300 grains of red oxide, with a little green oxide, which occupied the bottom of the retort. The pressure of the red oxide and sulphate of soda, by which the green oxide was covered, had prevented the disengagement of the hidrogen from this portion.

Proof that green oxide of iron does not acquire more oxigen from oxigenized muriatic acid.

In support of my opinion I shall add two facts, which, though less direct than those I have already given, are of considerable weight. When oxigenized muriatic acid is poured on a green solution, if the oxigen of the acid combined with the oxide of iron, a considerable extrication of caloric must take place, in consequence of its more dilated state in the acid, and more condensed or fixed state in the oxide, from which the greatest heat of our furnaces cannot expel an atom. But I convinced myself by several experiments, that the increase of temperature is scarcely perceptible. This slight evolution of caloric is consistent with the combination of the hidrogen, given out by the hidruret of iron, and the oxigen from the muriatic acid, because in both these combinations the gasses are nearly in the same state of dilatation as when they form water.

Proof that sulphuretted hidrogen does not deprive it of oxigen.

Lastly, if the action of sulphuretted hidrogen on a red solution of iron be merely to bring it back to the same degree of oxidation, as that in which the common green solutions are, the properties of both should be the same. On the contrary, the solutions rendered green by sulphuretted hidrogen rapidly change red on exposure to the air; and heated for a quarter of an hour they become entirely red: which is not the case with the common green salts recently made.

(To be concluded in our next.)

VI.

V.

Account of Mr. CURWEN's *Drill Horse Hoe, or Weed Harrow.**

SIR,

AS one great and most important advantage of drill husbandry proceeds from the opportunity of cleaning foul grounds, as also of breaking and loosening stiff soils, to give the power of extension to the roots of grain; whatever can facilitate these operations, will, I flatter myself, be deemed worthy of the attention of the Society. Advantage of drill husbandry.

Having hitherto found great difficulty and much labour necessary in accomplishing the cleaning of wheat and other grain, I have been led to make some experiments, and I am sanguine in my hopes, that the harrow I send for the inspection of the Society will be found to accomplish the purpose with greater ease and facility, than any thing at present in use. Difficulty of cleaning from weeds.

The simplicity and ease, with which it is worked, have enabled me, this season, to give my wheat crop, which exceeds one hundred acres, two cleanings, and at an expense of somewhat less than a shilling per acre each operation; a man and boy, with one horse, being able to clean above seven acres a day. The direction of the harrow, to prevent its injuring the grain, is effected by an alteration of the chain, by which it is attached to the wheels. The distance of the teeth from the centre tooth must be regulated by the width of the drills. In case they exceed a foot, the harrow should be broader, to admit of another row of teeth. To clean at nine inches, two inches and a half are allowed on each side of the centre tooth, by which means every part of the earth is cut between the rows of grain. The size and strength of the teeth must be regulated by the nature of the soil. The thing is so simple, that I hesitated laying it before the Society, till I was encouraged by persons, whose experience and knowledge are infinitely greater than my own. Harrow for this purpose.

The complete introduction of drill husbandry would, I conceive, be of great national importance, and under this Drill husbandry recommended.

* Transactions of the Society of Arts, for 1806.

conviction whatever can facilitate its operations may not be unworthy of attention, and will, I hope, excuse the liberty I have taken.

I have the honour to be, Sir,
Your obedient humble Servant,
J. C. CURWEN.

London, May 5th, 1806.

Explanation of the Engraving of Mr. Curwen's Drill Horse-Hoe, or Weed Harrow. Plate VIII. *Fig.* 1, 2.

Explanation of the plate.

Fig. 1. shows the carriage, within the shafts of which, A, the horse is placed: the carriage wheels are intended to be half the width of the butts or stitches, so that once going up, and once returning, will be sufficient to clear each butt from weeds.

B, The hoe, or harrow, which is attached to the carriage by the chains C C. The harrow may be raised higher, or sunk lower, or placed more on one side or another, as occasion may require, by altering the position of the chain, as will appear by an inspection of the plate.

DDDDDD, Six double rows of teeth, or knives, which are so placed in the frame, that each double row may pass up the interval betwixt the rows of corn, and cut or pull up the weeds, that grow in such intervals, without injuring the corn. These knives are strong, and have a sharp edge in front.

E E Are the two handles, by which the person who holds them may direct the knives, or teeth of the harrow, to pass in straight lines up the intervals.

Fig. 2. shows the underside of the weed harrow, that the positions of the double rows of the knives, and of the space left to prevent the corn being injured, may be more clearly seen.

Certificates in Favour of Mr. Curwen's Harrow.

The harrow clears all the weeds,

At the request of J. C. Curwen, Esq., I beg leave to state, that I have been present, when Mr. Curwen's harrow for cleaning drilled corn has been used, and have worked a little with it myself; that its effect appeared to me most highly beneficial in clearing away in the spring all the weeds,

weeds, that had grown during the winter among the wheat, without the least injury to the grain; and also in raising up the top soil, which had become sad and heavy, and thus enabling the spring shoot to take root more easily: and at the same time it covers the roots of the corn with fresh soil, which are often left quite bare by the washing of the rains in winter, and so subject to be killed by the frost. It also enables the farmer, to sow his barley much earlier than he could broad-cast, as it will both clear the corn previous to sowing the grass seeds, and afterward harrow them in. Its utility in every respect appeared to me so very great, that I was induced to adopt the plan of sowing my corn with the drill upon my fallows this spring, and have accordingly got a harrow made upon the model of Mr. Curwen's.

raises the top soil,

covers the roots of the corn,

and harrows in grass seeds.

I have the honour to be, &c.

J. D. B. DYKES.

Dovenby Hall, May 13, 1806.

SIR,

We whose names are hereunto subscribed do certify, that we have paid particular attention to a harrow, made use of in the farm of J. C. Curwen, Esq., for the purpose of harrowing between the rows of drilled grain. We conceive it of great utility; the expedition is undeniable, as upwards of seven acres can be done with ease in eight hours, with only a boy to lead the horse, and a man to regulate the harrow.

We are, Sir,

Your most obedient humble Servants,

THOMAS GAFF, *Merchant*, Workington Hall Mills.

MATTHEW FOSTER, *Farmer*, at Calva, near Workington.

Workington-Hall, May 19, 1806.

VI.

Account of a Drill Horse Hoe for Turnips, communicated by Mr. CHARLES WAISTELL.*

Drilling turnips decidedly preferable to broad-cast.

IN consequence of the premiums, which the Society of Arts has offered, and bestowed for several years, on the comparative culture of turnips, the drill practice has appeared so decidedly superior to the broad-cast method, that they have thought it unnecessary to continue them. At the same time they have given a figure and description of a useful drill hoe and harrow, with which they observe they shall probably finish the subject, and which therefore we shall lay before our readers nearly in Mr. Waistell's words.

DEAR SIR,

Hoe harrow for turnips and other wide drilled crops.

I HAVE ordered a new agricultural implement to be left for a short time at the Society's Repository for inspection. It is called a hoe harrow, and is, as its name imports, a hoe and harrow combined. For destroying the weeds, and pulverising the soil in the intervals of drilled turnips, and of other crops drilled sufficiently wide to be horse-hoed, I know not of any other implement of equal efficacy.

Its use.

It enables the farmer to cultivate those intervals as completely as a well wrought fallow, so long as the horse can travel therein, without injury to the growing crop. I know not who the meritorious inventor is. The first I saw was a few years ago at West Park, near Barnard Castle. This was brought from Carlisle by my brother, and many have been made from that pattern, and are now in use, and are highly approved of by farmers in the neighbourhood of Barnard Castle, where the turnip crops are now generally raised in drills about 27 inches apart. This mode was first introduced there about 23 years ago, before which time they were all sown broad-cast.

Near Barnard Castle turnips drilled at 27 inches.

An implement of husbandry, possessing such superior utility, as this hoe harrow seems to me to possess, is deserving of being made known as generally and as speedily as possible. I conceive this would be best effected through the

* Trans. of the Soc. of Arts for 1806.

medium

medium of your respectable Society, to whose notice I must entreat you to have the goodness to introduce this implement. Should they concur in opinion with me respecting it, I am persuaded, that they will give a plate and description of it in their next volume.

Convinced, that the fertility and productiveness of our arable grounds may be much increased by a more general practice of the horse-hoeing husbandry, I wish to see the practice of it advanced more nearly to perfection, as that must tend to promote its more general adoption. Horse hoeing recommended.

I am, Dear Sir,
Your very humble Servant,
CHARLES WAISTELL.

No. 99, *High Holborn.*

Explanation of the Engraving of Mr. Waistell's Drill Horse Hoe Harrow, Plate VIII. *Fig.* 3.

Fig. 3. shows the hoe harrow, to which the horse is to be attached by the upright iron *a*, in which are a number of holes, to admit the drag chain to be put higher or lower, as may be found necessary. This iron is at one end fixed firm in the fore part of the machine at *b*, and at the other end to the farther side, or wing, *c*. Explanation of the plate.

d, Is the nearer side or wing of the machine, and movable by a joint at *e*. This wing may by this mean be expanded or contracted, as the interval between the rows to be cleared of weeds may require.

f, A strong wedge-like tooth in the fore-part of the machine, to tear up the weeds, which are deep in the ground.

g, *g*, Other teeth more slender, fixed in the two wings or sides of the machine, and also intended to tear up weeds and loosen the earth.

h, *h*, *h*, Three triangular hoes. That which is in front has a strong iron fixed in its centre; the two others at the hinder part of the machine have the irons fixed at the farther corner of each. The intent of the centre hoe is to cut off the weeds in the middle of the interval; and of the other two, those on each side next the crop, and to lay all the weeds in a ridge-like form in the middle of the path, to dry and rot.

i, *i*, The

i, *i*, The two handles, by which the machine is managed.

k, A slender iron bar, with a peg and holes to direct the distance of the expansion or contraction of the machine.

l, A strong iron vice, which works in a grooved iron, fixed to the inner side of the wing *d*, and which, when screwed down, holds the machine firm at the distance of expansion wanted for use.

Fig. 4. Shows on a larger scale one of the hinder legs separate from the machine, and the manner by which it may occasionally be raised or lowered in the machine by a pin and holes.

VII.

On Capillary Action. By Mr. LAPLACE.*

Results of capillary attraction generalized.

BY considering the theory of capillary action in a new point of view, I have not only succeeded in simplifying it, but in generalizing the results, to which I had been led before by analysis. I had determined the elevation or depression of fluids only in circular capillary spaces, and between planes: but I shall here proceed to determine them, whatever these spaces may be, or whatever the nature of the surfaces by which they are included; supposing even in these spaces any number of fluids placed one above another; and I shall thence deduce the increase or diminution of weight, that bodies plunged in fluids undergo by capillary action.

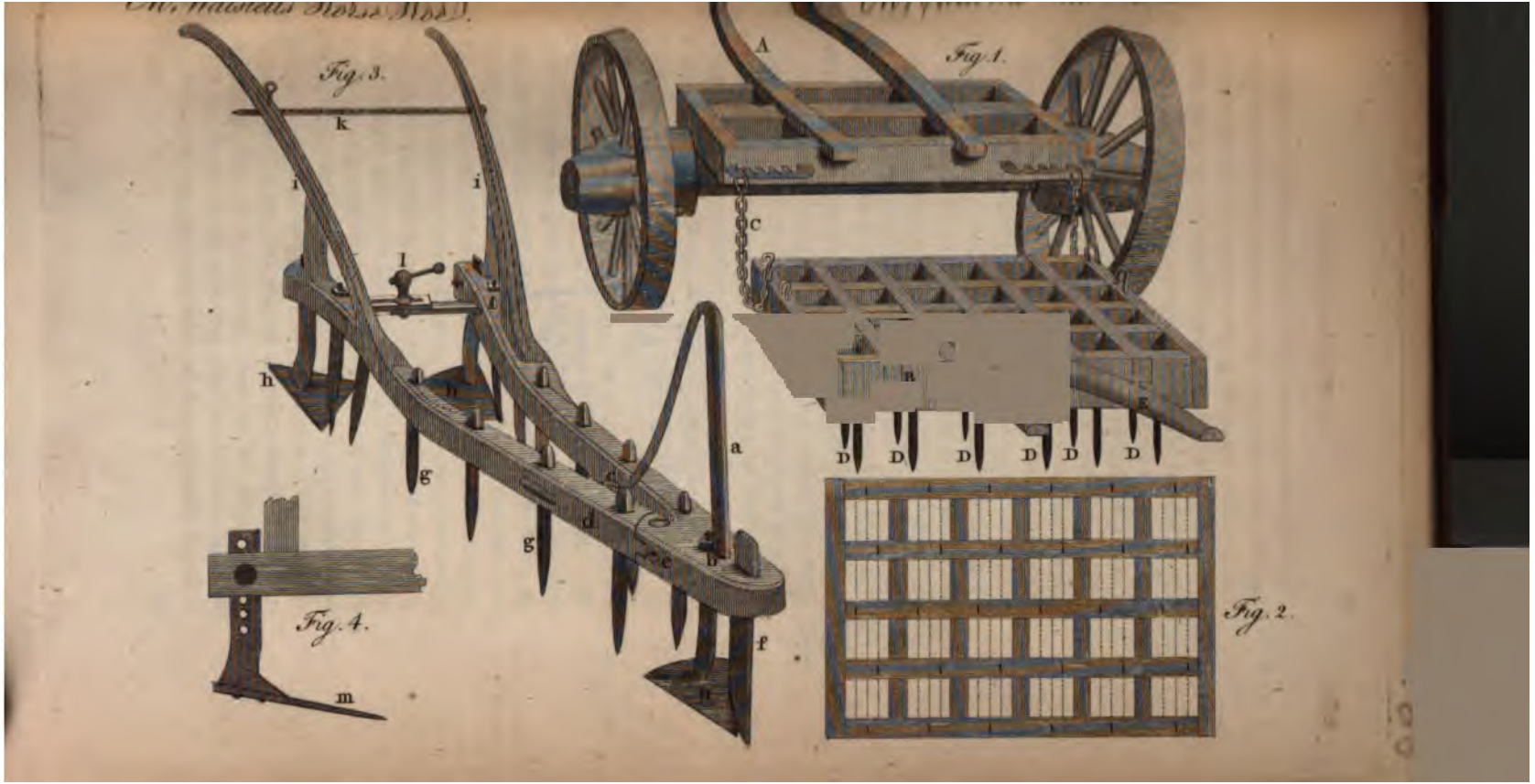
Determined for spaces of any figure, and for any number of fluids.

Affinities of substances to fluids deduced from their resistance to separation.

The combination of these results with those I have found by analysis has given me an accurate expression of the affinities of different substances to fluids, by means of experiments made on the resistance, that disks of different substances, applied to the surface of fluids, oppose to their separation. I dare venture to believe, that this will throw great light on the theory of affinities; for what I advance is founded on geometrical reasonings, and not on vague and precarious considerations, which ought to be strictly banished from natural philosophy; unless, imitating Newton in his Optics, we give them merely as conjectures calculated

* Journal de Physique, Vol. LXIII. p. 474, Dec. 1806.

to


Fig. 3.
k
i
i
l
h
g
g
d
a
b
c
f
m
Fig. 4.
A
Fig. 1.
C
D
D
D
D
D
D
Fig. 2.

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to guide us to farther searches, but leaving the merit of discovery almost wholly to him, who shall establish them on solid foundations by observation or analysis.

Supplement to the Theory of Capillary Action promised.

I intend to publish without delay, in a supplement to my Theory of Capillary Action, the analytical demonstrations of the theorems, which I have only mentioned in different numbers of this Journal. At the same time I shall give a new method of arriving at the fundamental equations of this theory. From these equations I shall deduce the general theorems, which I am now about to lay before the reader, demonstrating them by the direct consideration of all the forces, that concur in the production of capillary effects. It will appear, that the forces, on which these effects depend, do not stop at the surface of fluids; but that they extend through the whole of their interior, and even to the extremities of the bodies immersed in them; which establishes the complete identity of these forces with affinities.

Forces on which capillary attraction depends not confined to the surface, and identical with affinities.

Theorem.

"If we conceive any kind of prismatic tube, in a vertical position, with its inferior extremity immersed in a fluid of indeterminate quantity; the volume of fluid within, raised above the level by capillary action, is equal to the circumference of the interior base of the prism, multiplied by a constant quantity, which is the same for all prismatic tubes of the same matter immersed in the same fluid."

Demonstration.

To demonstrate this theorem, let us imagine, at the inferior extremity of the tube, a second tube, the infinitely thin sides of which are the prolongation of the interior surface of the first tube, and, having no action on the fluid, do not prevent the reciprocal attraction between the molecules of the first tube and the fluid. Let us suppose, that the second tube is at first vertical, that then it bends horizontally, and that afterward it resumes its vertical direction, retaining the same figure, and the same size, throughout its whole extent. It is evident, that, while the fluid is in equilibrio, the pressure in the two vertical branches of the canal formed by the first and second tube will be the same. But, as there is more fluid in the first vertical branch formed of the first tube and part of the second, than in the other vertical branch, the excess of pressure, that results from this, must be destroyed by the attractions of the prism and the fluid

for

for the fluid contained in this first branch. Let us analyse these different attractions with care, and first consider those that take place toward the lower part of the first tube.

Attractions toward the bottom of the tube.

For this let us conceive, that the base of the tube is horizontal. The fluid contained in the second tube will be attracted vertically toward the bottom, 1st by itself, 2dly, by the fluid surrounding the second tube. But these two attractions are destroyed by the similar attractions, that the fluid contained in the second vertical branch of the canal experiences near the surface of the level of the fluid. Accordingly we may leave them out of consideration here.—The fluid in the first vertical branch of the second tube will also be attracted perpendicularly upward by the fluid in the first tube. But this attraction will be destroyed by the attraction it exerts on the latter fluid: these two reciprocal attractions therefore may be set aside. Lastly, the fluid in the second tube will be attracted perpendicularly upward by the first tube, and hence this fluid will have a vertical force, which we shall denote by Q, that will contribute to destroy the excess of pressure owing to the elevation of the fluid in the first tube.

Forces acting on the fluid in the tube.

Let us now examine the forces, with which the fluid in the first tube is actuated. In its lower part it experiences the following attractions: 1st, it is attracted by itself; but the reciprocal attractions of the particles of a body impress upon it no motion, if it be solid; and we may conceive the fluid in the first tube to be consolidated, without any disturbance of equilibrium. 2dly, This fluid is attracted by the fluid in the interior of the second tube: but we have seen, that the reciprocal attractions of these two fluids destroy each other, and must not be taken into account. 3dly, It is attracted by the exterior fluid, that surrounds the second tube; and from this attraction results a vertical force acting downward, which we shall denote by $-Q'$. We prefix to this the sign $-$, to indicate, that its direction is contrary to that of the force Q. We shall observe here, that, if the laws of attraction relative to the distance be the same for the molecules of the first tube and those of the fluid, so that they differ only in respect to intensity; if we nominate these intensities in equal volumes ε and ε', the forces Q and Q' are

proportional to ϱ and ϱ': for the interior surface of the fluid surrounding the second tube is the same as the interior surface of the first tube, so that the two masses differ only in their thickness. But as the attraction of masses becomes insensible at sensible distances, the difference in their thicknesses can produce none in their attractions, provided these thicknesses be sensible. 4thly, and lastly, The fluid of the first tube is attracted vertically upward by this tube. In fact let us conceive this fluid divided into an infinite number of little vertical columns: if we draw a horizontal plane through the superior extremity of one of these columns, the part of the tube below this plane will produce no vertical force in that column. No vertical force will be produced therefore, but what is owing to the part of the tube above the plane; and it is evident, that the vertical attraction of this part of the tube for the column will be the same as that of the whole tube on an equal and similar column placed in the second tube. Thus the whole vertical force produced by the attraction of the first tube on the fluid it contains will be equal to that, which the attraction of this tube produces on the fluid contained in the second tube: this force therefore will be equal to Q.

Vertical force acting upward.

On combining together all the vertical attractions experienced by the fluid contained in the first vertical branch of the canal, we shall have a vertical force directed upward, and equal to $2Q-Q'$. This force must balance the excess of the pressure arising from the weight of the fluid raised above the level. Let V be its volume, D its density, and g its specific gravity, $gD\times V$ will be its weight. Thus we shall have $gD\times V=2Q-Q'$.

The base may be considered as a plane.

Now attraction being sensible only at imperceptible distances, the first tube acts sensibly only on columns extremely near to its sides: we may neglect the curvature of these sides therefore, and consider them as developed on a plane surface. The force Q will be proportional to the magnitude of this surface; or, which comes to the same thing, to the circumference of the base of the interior surface of the parallelopipedon. Thus, if we call this circumference c, we shall have $Q=\varrho\times c$; ϱ being a constant proportional to the intensity of the attraction of the matter of the first tube for the

the fluid. We shall also have $Q' = \varrho' \times c$; ϱ' being proportional to the intensity of the attraction of the fluid for itself. Therefore $V = \frac{(2\varrho - \varrho') \times c}{g D}$; which is the algebraic expression of the theorem to be demonstrated.

The constant quantity $\frac{2\varrho - \varrho'}{g \times D}$ may be determined by means of the observed elevation of the fluid in a very narrow cylindrical tube. Let q be the height, to which the fluid rises in this tube, and l the radius of its cavity: putting π for the semicircumference of which the radius is unity, we shall have nearly $V = \pi \times l^2 q$, $c = 2 l \pi$: the preceding equation then will give $\frac{2\varrho - \varrho'}{g \times D} = \frac{lq}{2}$, and consequently we shall have $V = \frac{lq}{2} \times c$.

If ϱ' exceed 2ϱ, q will be negative; and consequently, the elevation of the fluid changing to depression, V will be negative.

Let us put h for the mean height of all the fluid columns, that compose the volume V, and b for the interior base of the parallelopipedon: then we shall have $V = h b$, and consequently $h = \frac{lq \times c}{2b}$.

Proportions of bases, if similar figures.

When the bases of different parallelopipedons are similar figures, they are proportional to the squares of their homologous sides, and their circumferences are proportional to these sides.

If regular polygons.

If these bases be regular polygons, they will be equal to the products of their circumference multiplied into half the radius of the inscribed circle: the heights h therefore will be reciprocals to these radii. Denoting these radii by r, we shall have $h = \frac{lq}{r}$.

A square and a triangle.

Thus supposing two equal bases, one of which is a square, and the other an equilateral triangle; the values of r will be to each other as 2 to $3^{\frac{1}{4}}$, or nearly as 7 to 8.

The law confirmed in these by Gellert.

Mr. Gellert has published some experiments on the elevation of water in rectangular and triangular prismatic tubes of

of glass, in the Memoirs of the Petersburg Academy, vol. XII. These confirm the law, according to which the heights are reciprocals to the homologous lines of similar bases. Mr. Gellert farther concludes from his experiments, that the elevations of a fluid are the same in rectangular and triangular prisms, the bases of which are equal. But he admits, that this is not so certain as the law of the heights being reciprocal to the homologous lines of similar bases. In fact it has just been seen, that there is a difference of an eighth between the elevations of the fluid in two prisms, the bases of which are equal, and one of which is a square, the other an equilateral triangle. The experiments related by Mr. Gellert do not afford sufficient data, to compare their results exactly with the preceding theory.

A narrow parallelogram.

If the base of the parallelopipedon be a rectangle, the larger side of which is equal to a, and the other side, supposed very small, equal to l, we shall have $b=a\,l$, and $c=2\,a+2\,l$: consequently $h=\frac{lq\times(la+2\,l)}{2\,al}=q\times\left(1+\frac{l}{a}\right)$; and by neglecting $\frac{l}{a}$ we shall have $h=q$, agreeable to experience.

Where several fluids are concerned.

"If the indefinite vessel, in which the parallelopipedon is immersed, include any number of fluids placed horizontally one above another; the excess of the weights of these fluids contained in the tube, over the weight of the fluids which it would have contained without capillary action, is the same as the weight of the fluid that would rise above the level, if the vessel contained only that fluid in which the inferior extremity of the parallelopipedon is immersed."

In fact, the action of the prisms and this fluid on the same fluid included in the tube, is evidently the same as in the latter case. The other fluids contained in the prism being raised sensibly above its base, the prism has no action on either of them to raise or depress it. As to the reciprocal action of these fluids on one another, it would evidently be destroyed, if they formed a solid mass together, and this we may suppose without any disturbance of equilibrium.

Case of two fluids.

"If the vessel contain but two fluids, in which the prism is entirely immersed, so that its superior part is in one

one fluid, and its inferior in the other; the weight of the lower fluid, raised in the prism by capillary action above its level in the vessel, will be equal to the weight of a similar volume of the upper fluid, *plus* the weight of the inferior fluid, that would rise in the prism above the level, if there were no other fluid in the vessel, *minus* the weight of the superior fluid, that would rise in the same prism above the level, if the vessel contained this fluid only."

Demonstration. To demonstrate this, it is to be observed, that the action of the prism on the portion of the inferior fluid it contains is the same as if this fluid only were in the vessel: in both these cases then this fluid is drawn perpendicularly upward in the same manner, both by the attraction of the prism, and that of the fluid that surrounds the lower part of the prism; and these attractions united are equivalent to the weight of the volume of this fluid, that would ascend in the prism above the level, if it were alone in the vessel. In like manner the superior fluid, contained in the upper part of the prism, is drawn perpendicularly downward by the attraction of the prism and the fluid that surrounds this part, as it would be drawn downward by the same attractions, if the vessel contained only the superior fluid; and these attractions united are equivalent to the weight of the superior fluid, that would then rise in the prism above its level in the vessel. Lastly the column of fluids within the prism, which is above the level of the inferior fluid in the vessel, is drawn perpendicularly downward by its own weight, and perpendicularly upward by the weight of a similar column of the superior fluid. On combining all these forces, which must counterbalance each other, we shall have the theorem just announced. By the same principles we may determine what will take place, when a hollow prism is entirely immersed in a vessel filled with any number of fluids.

Where the base of the prism is not horizontal. In what has been said the base of the prism was supposed to be horizontal: but if it were inclined to the horizon, the vertical action of the prism on the fluid would still be the same. For a plane of a sensible thickness, having its lower part, the surface of which is terminated by a right line inclined to the horizon, immersed in a fluid, attracts this

this fluid parallel to its surface, and perpendicularly to the right line that terminates it, proportionally to the length of this line: but this attraction, resolved into a vertical force, is proportional to the horizontal magnitude of the plane. Hence it is easy to conclude generally, that, whatever be the figure of the base of the prism, its vertical attraction, and that of the exterior fluid on the fluid included in it, are the same as if the base were horizontal. The first theorem therefore will hold generally, if we understand by the circumference of the interior base that of the interior section, perpendicular to the sides of the prism.

The volume of fluid raised above the level in the inverse ratio of the sine of the inclination.

"If the prism, the lower part of which is immersed in a fluid in a vessel of indefinite size, be inclined to the horizon, the volume of fluid in the prism raised above the level of the fluid in the vessel, multiplied by the size of the angle of inclination between the side of the prism and the horizon, will be constantly the same, whatever this inclination may be."

In fact, this product expresses the weight of the volume of fluid raised above the level, and resolved into a force parallel to the sides of the prism: this weight, thus resolved, must balance the attraction of the prism and the external fluid to the fluid it contains; an attraction evidently the same, whatever may be the inclination of the prism; therefore the mean perpendicular height of the fluid above the level is constantly the same.

Ascent of a fluid between two parallelopipedons of the same material.

"If a parallelopipedon be placed perpendicularly in another parallelopipedon of the same material, and their inferior extremities be immersed in a fluid; putting V for the volume of fluid raised above the level in the space included between the two parallelopipedons, we shall have $V = \frac{(2\varsigma - \varsigma')}{g \times D} \times \left(c + c'\right) = \frac{l q^2}{2} \times \left(c + c'\right)$; c being the inner circumference of the base of the larger parallelopipedon, and c' the outer circumference of the base of the smaller."

Demonstrated.

This theorem is demonstrable in the same manner as the first. If the bases of the two parallelopipedons be similar polygons, the homologous sides of which are parallel, and placed all at the same distance, if we put l for this distance, the

the base of the space the two parallelopipedons leave between them will be $\frac{l \times (c+c')}{2}$: thus, h being the mean height of the fluid raised, we shall have $V = h\, l \times \frac{(c+c')}{2}$, and consequently $h = q$. We may determine too from the preceding principles what will take place, if the prisms be immersed wholly or partly in a vessel filled with any number of fluids, and in the case of their being inclined to the horizon.

Where they are of different materials.

"The data being the same as in the preceding theorem, if the two parallelopipedons be of different materials, put ς for the force of attraction which that of the greater has for the fluid, and ς_1 for the attractive force of that of the smaller, we shall have, $V = \frac{(2\varsigma - \varsigma')}{g\, D} \times c + \frac{(2\varsigma_1 - \varsigma')}{g\, D} \times c'$: so that, if we put q and q_1 for the elevations of the fluids in two very narrow cylindrical tubes of the same interior radius l, formed of these two materials respectively, we shall have $V = \frac{1}{2} l \times (qc + q_1 c.")$

This theorem too is demonstrable in the same manner as the first. It is easy to perceive, that by the same principles we shall obtain the volume of fluid raised above the level in a space included between any number of vertical planes of different materials.

Attraction on the outside of a prism.

It follows from the preceding theorem, that the volume V of the fluid raised by capillary attraction exteriorly to a prism immersed in a fluid at its inferior extremity, is equal to $\frac{2\varsigma - \varsigma'}{g\, D} \times c = \frac{1}{2} l\, q \times c$; c being the outer circumference of the prism. The increase of weight of the prism, owing to capillary attraction, is equal to the weight of this volume of fluid. It changes to diminution, if q be negative, and then the prism is raised by capillary action. If the base of the prism be a very narrow rectangle, of which a is the longer side, and l the shorter, putting i for its height, its solidity will be $a\, i\, l$, and its circumference, c, will be $2a + 2l$; and the volume V of fluid depressed by capillary action will be $a\, q\, l \times \left(1 + \frac{l}{a}\right)$. Putting k then for the

Hence increase of weight, or diminution,

ratio

atio of the specific gravity of the prism to that of the uid, the weight of the prism will be to that of the volume f fluid depressed as $i\,k : q \times \left(1+\frac{l}{a}\right)$. By suitably di- or equilibrium. ainishing i therefore, we may render the two weights equal, nd thus keep the prism at the surface of the fluid. From he preceding principles too we may determine the diminu- ion of weight of a body completely immersed in a vessel illed with several fluids.

If the end of a very slender tube be immersed perpendi- ularly in a fluid, putting l for the radius of the cavity of he tube, and q for the height to which the fluid is raised bove the level in it, we shall have, by my theory of ca- illary action, $l\,q = \frac{\cos.\ \varpi}{\alpha\, D}$; ϖ being the angle which the urface of the interior fluid forms with that part of the nner surface of the tube, which is in contact with it. When the fluid is depressed below the level, this angle xceeds a right angle, and then its cosine becomes negative, s well as q: but α is a constant quantity, which depends nly on the weight and action of the fluid on itself. By what precedes we have, $\frac{2\varrho-\varrho'}{g\,D} = \frac{l\,q}{2}$: therefore we shall ave $\cos.\ \varpi = \frac{2\alpha \times (2\varrho-\varrho')}{g\,D}$; (1.)

But it has appeared in the theory quoted, that, ϱ being ull, ϖ is equal to two right angles: which may be con- luded likewise from the analysis I shall give in a supple- nent to that theory, on the resistance that a very large ircular disk, applied to the surface of a fluid, opposes to ts separation from the fluid. From this analysis it follows, hat, i being the radius of the disk, supposed of the same natter as the preceding tube, this resistance is equal to $\frac{D \times \varpi \times i^2 \times \sqrt{2} \times \cos.\ \frac{1}{2}\varpi}{\sqrt{\alpha}}$: but it is clear, that it must e null, when ϱ is null, or when the disk has no action on he fluid; we shall then have $\cos.\ \frac{1}{2}\varpi$ null, which gives $\varpi = 2\varpi$, and consequently $\cos.\ \varpi = -1$: thus the equation 1) will give $\varrho' = \frac{g \times D}{2\alpha}$, and consequently $\frac{\varrho}{\varrho'} = \cos.^2 \times \frac{1}{2}\varpi$.

Resistance a disk opposes to separation from a fluid.

Hence

Hence the preceding expression of the resistance the disk opposes to its separation from the fluid, or, which comes to the same thing, of the weight necessary to raise it, becomes $2\pi \times i^2 \times \sqrt{g D \times \varrho}$. "For disks of the same diameter therefore, and different substances, the squares of these weights, divided by the specific gravities of the fluids, are proportional to the value of ϱ." Accordingly, by very accurate experiments on the resistances opposed by disks to their separation from the surfaces of fluids, we may determine their respective attractions for those fluids.

Attraction of a substance for a fluid determinable from its adhesion to the surface.

Two important observations are here to be made: the first is, that ϱ expresses the action of a plane of a sensible thickness on a fluid plane of a sensible thickness parallel to it, and touching it by the right line, that terminates one of its extremities; whatever be the laws of the attraction of the molecules of the fluid for those of the plane, and for each other, even in the case where these laws are not expressed by the same function of the distance. But if this function be the same, then the values of ϱ and ϱ' are proportional to the respective intensities of the attractions; or, which comes to the same thing, to the constant coefficients, which multiply the common function of the distance, by which the law of these attractions is represented; but these values are relative to equal volumes.

To show this, let us conceive two capillary tubes of the same diameter and different substances, but in which a fluid rises to the same height. It is clear, that, if in these tubes we take two equal volumes, infinitely small, and similarly placed, with respect to the interior fluid, their action on this fluid will be the same, and one may be substituted for the other. But to have their attractions in equality with the masses, the attractions of equal volumes must be divided by the specific gravities: the values of ϱ and ϱ' therefore must be divided by the respective densities of the different substances.

The second observation is, that the preceding results suppose ϱ less than ϱ': for, if ϱ exceeded ϱ', the fluid would unite intimately with the disk with which it was in contact, and thus form a new disk, the surface of which in contact with the fluid would be the fluid itself. But as by the preceding formula we may determine the resistance, that such a disk

a disk would oppose to its separation; we may be certain, that ε is less than ε', if the resistance opposed by a disk be less than the resistance thus calculated.

XIV.

Letter from Mr. DELAVILLE, *M. D. of Cherbourg, to Mr. Vauquelin, Member of the Institute, on the Oxidation of Metals, and particularly on that of Lead*.*

SIR,

I HAVE undertaken, and pursued as far as my occupations would allow me, some experiments on the oxidation of metals, particularly on that of lead; and though the results I have obtained are such as to inspire me with a wish to push my inquiry still farther, as the publication of these results, which I conceive to be not yet known, at least to many, may throw some light on the theory of oxidation in general, and contribute to render more economical the oxidation of lead in particular, as well as the preparation of some salts, that have this metal for their base, I shall do myself the honour of sending you a short account of these results, and of the means I adopted to obtain them. If like me you think them new, at least in some respects, I beg you would give them that sort of publicity, that may appear to you most suitable. Experiments on oxidation of lead.

It is known, that, in cleaning bottles, when a small quantity of shot is shaken in water, the friction in a short time separates particles of lead, which, being suspended in the water, render it turbid, and give it a slate gray colour. If the agitation be carried farther, the particles suspended in the water become of a lighter gray; and by continuing it they grow whitish, and at length of a pretty fine white. Shot oxided in washing bottles.

This oxide of lead has such a tendency to unite with carbonic acid, that on being exposed to the air, when taken out of the air, it is covered almost immediately with The oxide powerfully attracts carbonic acid.

* Annales de Chimie, Vol. LXVIII. p. 92.

a pellicle of a brilliant white, which appears to be nothing but carbonate of lead.

May be kept under water unchanged,

If kept under water, this oxide of lead undergoes no perceptible change, whether it be exposed to the light, or defended from it. But if it be kept ever so little time in a

but exposed to air and light becomes yellow, and then red.

flint glass phial with a little water only, it is found to attach itself to the sides of the phial above the surface of the water; and if the phial be exposed to the rays of the sun, that portion of oxide acted upon by the light changes successively from white to yellow, and from yellow to red, thus furnishing massicot and minium.

Changed to massicot and minium by heat.

If the white oxide be placed over a fire in a glass capsule it changes in a short time, from white to yellow, and from yellow to red, like that exposed to light.

Easily and cheaply manufactured.

This oxide may be manufactured in quantity, and at little expense, so as to lessen the cost of certain preparations in which it may be employed. It may likewise be used as it is in painting.

Method of making it.

The following is the method I have employed to obtain this oxide. In a leaden barrel I enclose a certain quantity of small shot, with as much water as equals about one fifth of its capacity, leaving the rest full of air. This barrel is turned round by means of an axis fastened to each end. It is obvious, that it might easily be kept in continual motion by a stream of water.

To renew the air in the barrel, I introduce leaden tubes at various parts of its circumference, soldered to the sides so that no water can escape, and reaching internally to the axis of the barrel, while the extremities are a few inches above its surface*.

* This must make an unnecessary addition to the weight of the machine, and cost of materials, at the same time that they must be liable to injury. They would admit the outer air equally well, if the external aperture were level with the surface of the barrel. T.

IX.

An Essay on Instinct, read to the French National Institute, by Mr. Dupont de Nemours *.

THOUGH Descartes would have brutes to be mere machines, it is now the general opinion, that they are conscious of their sensations, and that their actions are determined by feelings of pleasure and pain; that they have a good memory; that from repeated experience they form general notions, founded on a sentiment of analogy; that they are guided by the pleasure or pain, which they are thus enabled to foresee, and this frequently in spite of the actual impulse of present pleasure or pain; and finally that these means, well managed, may be employed by man to educate them, and lead them sometimes to acquire a habit of executing with wonderful precision very difficult actions, and even some to which their structure seems not adapted.

Animals possessed of consciousness, and act upon it.

Neither does any philosopher doubt, that animals have various modes of expressing their wants and passions; and that those of a superior order, or which approach us in their organization, learn the signification of several of our words, which they obey without mistake.

Are capable of expressing their wants and passions.

But independent of these faculties, which resemble ours except in degree, and in which the different classes of animals differ from each other as much as some of them from us, naturalists have imagined they discern in certain species other faculties, which appear to them essentially different, and to which they have given the name of instinct.

Supposed likewise to have instinctive faculties.

These are certain actions necessary to the preservation of the species, but frequently altogether foreign to the apparent wants of the individuals, and often very complex; which we cannot attribute to reason, without granting them a degree of foresight and of knowledge, that every one would hesitate to admit. Neither can they be attributed to imitation; since it appears impossible, that the individuals by which they are practised, can have thus learned them, and yet those of the same species constantly practice them nearly in the same manner. And it is no less remarkable, that the

Some of their actions difficult to ascribe to reason, or imitation.

These most re-

* Magazin Encyclopedique, February, 1807, p. 437.

markable in animals apparently least intelligent.

actions which bear no relation to the degree of ordinary understanding are more singular, more intelligent, and more disinterested, in proportion as the animals that execute them belong to classes of a lower order, and in every thing else more stupid. It is among the insects, molluscæ, and worms, that we observe the most admirable instincts. It seems as if instinct and reason were two faculties made to compensate and supply the want of each other; as on other occasions fecundity compensates the want of strength or long life. It is even by a due balance of reason, instinct, and physical qualities, as acuteness of the senses or bodily strength, that the species are continued.

Balance of reason and instinct.

These actions ascribed to an internal impulse.

Naturalists have imagined therefore, that animals endued with instinct perform their peculiar actions by virtue of an internal impulse, independent of experience, foresight, education, and all exterior agents; in other words, that it is their organization, which of itself determines them to act thus. This conclusion has been adopted by almost all observers: and if they have differed, it is only in explaining the manner, in which the organization can impart this determination. The following is one of these hypotheses.

Hypothesis.

The want or desire of a certain action can be occasioned only by sensations, or remembrances of sensations; in a word by images. It is not necessary, however, that a sensation should arise from without, for every external sensation requires interior movements of the brain and nerves, without which it would not have taken place: but these interior movements may originate in the organs themselves, without any external action, as is frequently the case in reverie, and in various diseases; nothing therefore prevents certain animals from being so organized, that internal movements shall regularly arise in them capable of producing certain sensations or images, and that these images shall imperiously determine their will to certain actions.

This does not lead to the doctrine of innate ideas,

This hypothesis appears to have nothing in common with that of innate ideas, the object of which is only general or abstract ideas: for they, who justly deny, that the general ideas of man are innate, have never pretended to assert, that man cannot have sensations from interior movements of his own organs, and without the intervention of external bodies; an assertion, that daily experience would have refuted.

Neither

Neither has it any thing in common with materialism; for, whatever idea we entertain of the intimate nature of the sentient principle, we are obliged to confess, that it experiences sensations only through the medium of the brain and nerves. materialism.

Finally, neither is it more closely allied than any other to fatalism: for, every action being determined, either by a present sensation, or by the recollection of a past sensation, or by the hope or fear of a future sensation, whether these sensations be external or internal does not alter the state of the question. or fatalism.

Mr. Dupont however appears to have been induced, to reject every sort of instinct indiscriminately, chiefly by the fear of splitting against one of these rocks. Mr. Dupont rejects instinct.

He begins by showing, that the actions of animals of the higher orders, as quadrupeds and birds, result from a combination of experience with their corporal faculties. In this there is no difficulty, as it is a point on which all naturalists are agreed. He then endeavours to explain physically how these animals, and children themselves, learn to suck. He shows, that several species are capable of uttering sounds sufficiently numerous to form a very complicated language; and he asserts, that he has observed them employ some of these sounds under circumstances so similar, as to leave scarcely any doubt of their attaching to them a fixed signification. His observations on this head are very interesting. His system.

He likewise endeavours to prove, that various species are capable of improving their operations under certain circumstances: though perhaps the naturalist will object to him, that he has sometimes taken different species for the same species improved. Thus the architect beaver of Canada is not precisely the same as the burrowing beaver of the Rhine; and the social spider of Paraguay is not the same with our solitary spiders. Animals capable of improvement.

It may be supposed, that the greatest difficulty Mr. Dupont has to encounter is in explaining, how insects have learned those wonderful precautions, with which they provide a shelter and proper nourishment for the egg, which they and sometimes even others are about to lay, and the maggot, that is to be produced from it; though frequently these Difficulty in the case of insects providing for their young.

these insects have never seen, and never will see again, the egg, or a similar maggot; and the wants of the maggot have not the least resemblance to those of the insect that labours for it.

Curious instance of the sphex.

Among thousands of instances, that might be adduced, Mr. Dupont has chosen but one, that of the sphex, or ichneumon wasp. In this he cannot be accused of having taken an easy example. The following is its economy. During its existence as a perfect insect, it lives entirely on flowers. When it is ready to lay, it forms a cylindrical hole in clayey sand, and deposits an egg at the bottom of it. It then seeks on cabbages a small green caterpillar, on which it never preyed before; pierces it with its sting, so as to weaken it to such a degree, that it may be unable to resist the maggot, which is afterward to issue from the egg and feed upon it, yet not so as to kill it, that it may not putrefy; rolls it up in a circle; and lays it in the hole upon the egg. It successively proceeds in quest of eleven more of these, which it treats in a similar manner. It then closes the hole, and dies. The little maggot is hatched, devours the twelve caterpillars in succession, and changes to a chrysalis in the hole. As soon as its final metamorphosis is completed, it issues from its subterranean abode a winged insect, to enjoy itself among the flowers, till it is ready to lay, when it repeats the operations its mother had performed before it, and with caterpillars of exactly the same kind.

How explained by the author.

Mr. Dupont supposes in his explanation, that the perfect insect retains the remembrance of the sensations it experienced in the state of a maggot, though its form and organs are totally changed. He must likewise suppose, though he does not expressly say it, that the sphex can afterward distinguish by the sight* the caterpillar, and the sand, of which it acquired a knowledge only by feeling, and this by its ancient feeling of a maggot; for the maggot is blind, it lives under ground, and when it there becomes a winged insect the caterpillars are devoured. Lastly, Mr. Dupont dares

* This is not necessary: it may distinguish them by the smell, or in some other way; for it by no means follows, that, because man has only five senses, an insect has no more. W. N.

not

not admit, that the sphex foresees the egg it lays will produce a maggot, and will have need of all it provides for it: according to him it does this merely for amusement, in imitating what it perceived in its infancy.

X.

Observations on the Sulphurous Acid; by Mr. PLANCHE. *Read to the Society of Pharmacy, November the 15th,* 1806 *.

MR. BERTHOLLET made known several remarkable properties of sulphurous acid, in two excellent Memoirs, read to the Academy of Sciences in 1782 and 1789. In the year 1796, Messrs. Fourcroy and Vauquelin read a much more extensive memoir on the same subject at the Institute, in which they gave a more complete history of this acid, and of its different combinations.

Changes produced by sulphurous acid on sirup of violets reddened by other acids.

I have considered with great attention the labours of these learned chemists, but among their numerous experiments I do not find any, which actually relate to the object of my present investigation; the changes that liquid or gaseous sulphurous acid occasions in sirup of violets reddened by different acids, and the contrary. This property of the sulphurous acid I am more eager to make known, as it may furnish matter for interesting reflections on the theory of acids in general.

The acid prepared by sulphuric acid and mercury.

The sulphurous acid I employed in my experiments was prepared by decomposing very pure sulphuric acid by means of mercury equally pure. In its preparation I followed the process of Berthollet. My sirup of violets was of a very fine blue, without any mixture.

Restored the blue that had been changed red.

Experiment 1. Sirup of violets, diluted with eight parts of distilled water, and coloured red by nitric, muriatic, sulphuric, phosphoric, or acetic acid, resumed its blue colour on the addition of liquid sulphurous acid. The colour was not quite so intense indeed, as before it was changed red, but it had no mixture of the latter colour.

Annales de Chimie, Vol. LX. p. 254. December, 1806.

Exp.

Reddened again by the other acids.

Exp. 2. The acids above mentioned, added by little and little to the blue liquor, restored its former red colour immediately; the acetic acid excepted, the action of which was slower by a few minutes, and it required to be added in a pretty considerable quantity.

Exp. 3. Sirup of violets diluted with a similar quantity of water, and coloured red by oxalic, citric, tartarous, and acetous acids, had its blue colour equally restored by adding a few drops of liquid sulphurous acid: but on the subsequent addition of these acids they exhibited some peculiar properties, which I shall proceed to mention.

Oxalic acid.

1. The oxalic acid in a small dose produces at first no change. It must be added in considerable quantity, to make the liquor assume a violet hue; and several hours elapse, before it resumes its red colour.

Tartarous, citric, and acetous.

2. The tartarous, citric, and acetic acids, mixed in any proportion with the blue liquor, cannot again make it red, even though it remain exposed to the air for twelve hours.

The sulphurous acid continues to diminish the colour.

3. In these three experiments the blue colour continues to decrease; which indicates, that the sulphurous acid still enjoys its property of destroying colours, notwithstanding the excess of the other acids. All these experiments were made in glass vessels open to the air: but it was necessary to ascertain, whether this agent had any influence on the colour of the different mixtures; for which purpose I repeated the same experiments in bottles closely stopped, and operating as quickly as possible.

Experiments made in stopped Bottles.

Exclusion of air did not prevent the action of the sulphurous acid,

Exp. 4. Into nine flint glass bottles with stopples I put sirup of violets diluted with water as above, and reddened by the same acids, and ticketed them. Into each phial I dropped liquid sulphurous acid, till the blue colour was restored, taking care to shake the mixture well after each drop, and observe the change induced in its colour. This I did with all the nine phials in succession; and, stopping them as I did it, I left them at rest for six hours. In this space of time I observed the blue had lost a little of its intensity, without being affected with any tinge of red.

but modified the subsequent

Exp. 5. I had next to examine, whether the acids employed

ployed in the preceding experiments had equally the property here of reddening the sirup of violets, that had been rendered blue by the sulphurous acid, and the following were the results. action of the others.

With the nitric, muriatic, sulphuric, and phosphoric acids, the blue liquor changed to a vinous red:

With the acetic, to a light violet:

With the oxalic, to a pale rose colour:

With the tartarous, citric, and acetous, mixed in a very large proportion, there was no tint of red, but a remarkable diminution of the intensity of the blue.

Experiments with Sulphurous Acid Gas.

Exp. 6. It is well known, that the sulphurous acid in the state of gas acts with much more energy than in the liquid state. Accordingly I was desirous of examining its action on sirup of violets, diluted as before, and changed red by the same acids. I disposed my apparatus exactly in the same manner as for preparing sulphurous acid. As soon as the second phial, three parts filled with distilled water, was saturated, I opened a communication between it and a third, filled with a mixture of water and sirup of violets reddened by sulphuric acid. A few bubbles of the sulphurous acid gas were sufficient to restore the blue colour of the liquor. To this I substituted another phial, filled with a similar mixture, except that it had been reddened by a different acid: and thus I continued, till mixtures reddened by all the acids mentioned in the first experiment had been subjected to the action of the gas. I did not observe any very sensible difference between them; but it appeared to me, that the colour was less weakened by the sulphurous acid gas, than by the liquid sulphurous acid. Experiments with the gas. It did not appear to act more powerfully.

The slight difference, however, may have depended on the greater quantity of coloured liquor in the latter experiments, and the facility with which the effects of the gas could be observed, and its action governed.

These experiments repeated with sulphurous acid obtained by the medium of charcoal, or that of sugar, afforded similar results. The acid prepared in different ways acted the same.

SCIENTIFIC

SCIENTIFIC NEWS.

French National Institute.

Prize questions. Phosphorescent substances.

THE following prize questions are proposed for the year 1809. A considerable number of substances, under different circumstances, diffuse a phosphorescent light, more or less vivid, and more or less durable. Such are the fluate of lime, and some varieties of phosphate of lime, when thrown in powder on a heated body; the Bolognian phosphorus, when, after having been exposed to light, it is carried into a dark place; certain sulphurets of zinc, when rubbed with a hard substance, or even with a quill; rotten wood, certain fishes, and other animal substances approaching to putrefaction, when in the dark; &c. The Class of Mathematical and Physical Sciences therefore proposes as the subject of the physical prize, which it will adjudge in the public meeting of the first Monday in January, 1809, the following question.

"To ascertain by experiment what relations subsist between the different modes of phosphorescence, and to what cause every kind of it is owing, excluding from the examination the phenomena of this class that are observed in living animals."

The prize will be a gold medal of the value of 3000 fr. (125*l.*); and the papers must be delivered at the secretary's office before the 1st of October, 1808.

The term of the following question is prolonged from the 21st of March to the 1st of October, 1807, in consequence of the change made in the period of the annual meetings, which will prevent a decision on the papers from taking place before January.

Hibernation of animals.

"To determine by anatomical and chemical observations and experiments, what are the phenomena of the torpidity, that certain animals, such as marmots, dormice, &c., experience during winter, with respect to the circulation of the blood, respiration, and irritability; and to investigate the causes of this sleep, and why it is peculiar to those animals."

Messrs.

Messrs. Bosc, Silvestre, and Palisot de Beauvois have been elected members of the institute. Mr. de Beauvois, who succeeds Mr. Adanson, merited his success by his travels in Africa and America, the fruits of which were the Floras of Owerra and Benin, already published, and that of the United States, which he is preparing for the press, as well as by researches concerning the cryptogamia class. These researches have not only furnished descriptions of new species and genera, but more particularly a system of the fecundation of mosses and mushrooms, of which we shall give a brief outline. New members of the French Institute.

Amid that dust of the capsules of mosses, which Hedwig considered as the seed, is a kind of nucleus, or little axis, more or less swelled, called by botanists the columella. In this nothing has been observed but a parenchyma, more or less cellular; and so it is represented repeatedly by Hedwig. In this Mr. de Beauvois says he has perceived very small grains, which he believes to be the true seeds; and the other dust, that fills the capsule around it, he supposes to be the pollen. When the capsule is ciliated, the setæ by their motion compress the pollen against the seeds, to fecundate them, at the moment when they are about to escape. Beauvois's system of the fructification of mosses,

With respect to mushrooms his opinion is similar. The multitude of little grains, or dust, spread over the gills, or other parts of some, and included in others, as the lycoperdons, which have been supposed to be seeds, are according to him the pollen; which in the same manner fecundates the true seeds, that are contained within the gills, or part covered with this pollen, just as they burst from these. and of mushrooms.

In consequence of this opinion, Mr. de Beauvois has taken the liberty of substituting the term ætheogamia, or uncommon fructification, to that of cryptogamia, to which the class is equally entitled, even on his own hypothesis, and which is certainly more scientific. New name of the class.

Part of his *Prodrome d'Æthéogamie* is published, in which he has announced his distribution of the mosses. In this he has some claim to impartiality; for while in forming his genera he rejects the sexual organs of Hedwig, he takes no account of the columella, which he considers as the pistil. In the second part, which is about to appear, he has reduced Prodromus of the class Æthogamia.

duced the number of genera of the mushrooms to sixty, which he distributes into six orders.

Seeds of the parasitic fungi pass through the epidermis of plants.

In a subsequent essay he asserts, that he has seen on young plants particles appearing to him similar to the seeds of parasitic funguses, that are accustomed to unfold themselves in the substance of the plants, underneath the epidermis: and hence he concludes, in opposition to Mr. Candolle, that these grains pass through the epidermis, to lodge themselves beneath it.

Mushrooms increasing by horizontal layers.

He treats more largely on certain mushrooms, that grow by layers from the top downwards, contrary to other vegetables. This observation is not new; but his opinion is; for he considers each layer as a new mushroom, produced from the seed of the layer above it.

The raphia of Owerra different from the sago tree.

He has likewise shown, that the flowers of the raphia of Owerra differ too widely from those of the sago tree of the Moluccas, to continue them in the same genus of palms.

Mr. de Candolle.

An unsuccessful competitor of Mr. de Beauvois was Mr. de Candolle, who, though young, has distinguished himself in vegetable physics, as well as in other branches of botany. Among his labours may be particularly noticed his observations on the action of artificial light, which, operating at first imperceptibly, at length effects a total change in the habits of vegetables: on the cortical pores: on the production of oxigen gas by green lichens, which has been denied, but the reality of which he has proved: and on the vegetation of mistletoe, which really attracts the sap of the apple-tree, but cannot draw up water, in which it is directly immersed; a fact of importance with respect to the cause of the ascent of the sap in plants.

Action of artificial light on plants.

Production of oxigen by lichens.

The mistletoe attracts sap, but not water.

Parasitic funguses.

Mr. Candolle presented three memoirs to the class on the occasion. The first was on those parasitic funguses, that develop themselves beneath the epidermis of plants, and cause several fatal diseases, as the blight in corn. (See Journ. vol. X. p. 225.) It has been supposed, that the seeds of this plant were introduced through the pores of the epidermis: but as coloured liquids traverse these pores with difficulty, and simple application does not inoculate the plants with these diseases, he conceives the seeds to be introduced by the roots with the nutritious juices, and circulate

Their seeds introduced into plants by the roots.

late with them till they arrive at places suitable for their developement. He compares them in this respect to intestinal worms, which can subsist only within the bodies of other animals. From this theory, and the observation, that each parasitic fungus is capable of being propagated only in plants of the same family, he deduces rules of which the farmer may avail himself to avoid the contagion. Eighty-four species of these fungi were already known, and Mr. Candolle has added more than a hundred to the number. Near 200 species.

In a memoir on algæ he has shown, that these marine plants have no true roots; that there is no trace of vessels in their organization; that their whole surface absorbs moisture; and that the greener they are the more oxigen gas is extricated from them by light He adds, that the little grains, hitherto considered as their seeds, are merely capsules, and contain seeds much smaller, enveloped with a viscous matter, which fixes them where they are to grow. Algæ.

Another unsuccessful competitor was Mr. du Petit-Thouars, who resided a long time in the isles of France and Bourbon, and visited Madagascar. He has begun to publish a Flora of these places, rich in singular plants. His observations on the germination of the *cycas*, or sago tree, which some have considered as a palm, others as a fern, have convinced him, that it ought to constitute a separate family, equally distinct from both. Du Petit-Thouars. The sago tree distinct both from the palms and the ferns.

Mr. Ventenat has published the 20th number of his Garden of Malmaison, but ill health has obliged him to take some respite from his labours.

In Mr. de la Billardiere's 23d number of his Flora of New Holland, he describes a tree by the name atherosperma, which he considers as belonging to the family of ranunculi, that may probably become useful in France. Its nuts have the taste and smell of nutmegs, and it appears capable of enduring the climate very well. A fruit resembling the nutmeg capable of growing in France.

Mr. von Humboldt, and his fellow traveller, Mr. Bonpland, continue the publication of the plants they observed in South America. The genus melastoma alone furnished them with so many new species, that they might have filled a separate work with them. Von Humboldt.

They

The condor. They have not less enriched the science of zoology. The condor has never before been so accurately described. Its size has been much exaggerated. It scarcely exceeds a metre (3 feet 3 in.) in height, or three or four in spread of wing. Its general colour is blackish brown; and round the lower part of the neck is a collar of white feathers. The male is distinguished by a fleshy crest on the top of the head, and a white spot in the wing.

Electrical eel of Surinam. They likewise made some curious observations on the gymnotus electricus. In the water it is capable of giving such a shock to a horse, as to stun it, so that it falls down, and is in danger of being drowned. Mr. von Humboldt, putting both his feet on one just taken out of the water, felt an acute pain, that did not entirely go off the whole day. Slighter shocks induce a peculiar trembling, a kind of twitching of the tendons, different from those of common electricity. The pain is more like that produced by galvanizing a wound.

Mr. Tenon has given an important continuation of his Memoirs on the Dentition of the Horse.

Fossil remains of lost animals. Mr. Cuvier continues his inquiries concerning the animals, that appear to have been destroyed by some revolutions of the globe. He has described five in the last half year, all of the genus mastodontes: the characters of which are to have tusks and a proboscis, and their grinders furnished with conical protuberances arranged in pairs. In the plaster quarries of Montmartre a skeleton of one of the species described by Mr. Cuvier has lately been dug up nearly entire.

Genus mastodontes.

Beauvois's insects. Mr. de Beauvois has published the third number of his insects collected in Africa and America.

Mr. Vauquelin has instituted an accurate analysis of the iron ores of France, their products, the fluxes employed, and the scoriæ, with a view to ascertain the causes of the defective qualities of the iron. These he attributes to remains of chrome, phosphorus, and manganese. He observes too, that this compound, sublimed in the furnaces, bears much resemblance to that of the stones that have fallen from the atmosphere, except that these contain nickel also; and

Iron rendered hot and cold short by chrome, phosphorus, and manganese.

and he conceives it not impossible, that the particles carried up from our furnaces may contribute in some degree to their formation.

Sparry iron ores.

Messrs. Descotils and Hassenfratz too have been examining the sparry iron ores; and the former ascribes the infusibility of some of them to magnesia, which the latter denies.

A carbonate of manganese confounded with them.

Mr. Lelièvre has described a mineral, that has been hitherto confounded with the iron spars, which he finds to consist of more than half oxide of manganese, near one third carbonic acid, only eight per cent of iron, and two and half per cent of lime. He has likewise described a stone, which he found in the island of Elba. This contains more than half oxide of iron, a little oxide of manganese, and the rest is silex and lime. Its crystalline nucleus is a prism with a rhombic base, its colour black and opake, its hardness a little inferior to that of feldtspar, its specific gravity 4. Mr. L. has named it *yénite*, from one of the most memorable events of this century. [From the battle of Jena we presume; a mode of composing new names, in which we trust he will be followed by few of the real friends of science.]

Yenite.

Ancient pewter.

Mr. Baraillon having discovered some ancient pewter vessels in digging among the ruins of the Roman town of Neris, near Montluçon, they were analysed by Mr. Anfrye, inspector general of assays at the mint, and found to contain thirty-five per cent of lead.

Method of freeing alum from iron.

To the different modes of freeing alum from iron Mr. Seguin has added another, founded on its difference of solubility when contaminated with iron, and when pure. By dissolving sixteen parts of common alum in twenty-four of water, and crystallizing, he obtains fourteen parts of alum as pure as the Roman, and two nearly the same with that of Liege. This process might be adopted in the first instance in manufacturing alum, so as to enhance its value one third.

Heat a vibratory motion.

It is known, that count Rumford adheres to the old theory of heat being simply a vibratory motion of the particles of bodies. As a strong objection to this has been adduced the production of heat by condensation, as if some sub-

stance were mechanically pressed out of the pores of bodies thus diminished in bulk. In answer to this he has shown, that some cases of condensation are accompanied by the production of cold. Thus solutions of several salts, being mixed with pure water, lose at the same time both bulk and heat. The generation of cold by dissolving salts is a well-known phenomenon, and has been ascribed to the necessity of a solid's absorbing heat when it is converted into a liquid: but here this explanation will not apply, as the solid is already dissolved, before it is mixed with the water.

Heating water by steam applied to soap-boiling.

Count Rumford has likewise made a very happy application of the process of heating water by steam to the manufacturing of soap. He has succeeded in boiling soap to a proper degree by its means in six hours, which in the common mode requires sixty. He conceives, that this saving of time is partly owing to the concussions given to the mixture of oil and lie by the heated vapour forced into it, and there suddenly condensed.

Improvement in boilers and evaporators.

He has also made a new improvement in boilers for heating or evaporating liquids. This consists in adding to their bottoms several tubes, which descend into the flame, so as to be surrounded by it on all sides; thus increasing the surface of the bottom, without adding to its diameter.

(In our next we shall give an account of the Transactions of the Mathematical Division of the Class.)

Correction.

The *Camera Lucida* described in our Journal, No. 71, p. 1, is sold not only by Mr. Newman, but also by Messrs. P. and G. Dollond, St. Paul's Church Yard.




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Nicholson's Philos. Journal Pl. III

Apparatus for triturating Quicksilver.

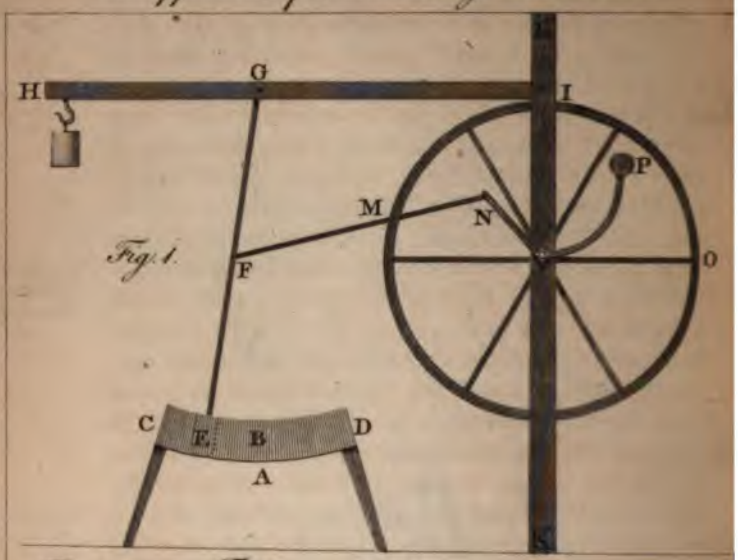


Fig. 1.

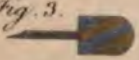
Fig. 2.



Fig. 4.



Fig. 3.



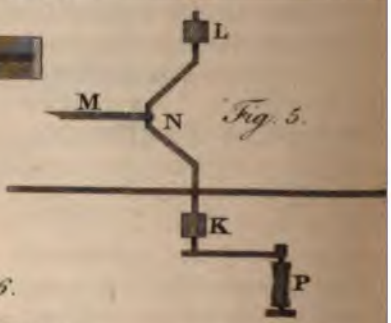
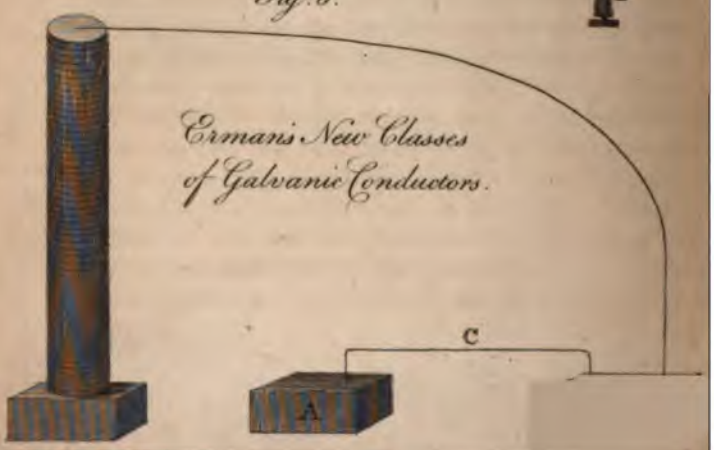


Fig. 5.

Fig. 6.



Ermans New Classes of Galvanic Conductors.

A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

SUPPLEMENT TO VOL. XVII.

ARTICLE I.

Description of a Machine for triturating and combining Quick-silver with other Substances, by a CORRESPONDENT.

SIR,

THE difficulty and tediousness of the process of combining pure mercury with mucilaginous or fat substances by trituration with a pestle and mortar, so as to bring it to that state of extreme division, in which alone it can exert all its efficacy as a medicine, are well known; on account of which some have recommended the use of a small quantity of flowers of sulphur, or of sulphuretted oil, others that of rancid fat, each of which operates by its chemical action on the mercury, and so far is at variance with the original intention. In forming mercurial plasters the use of some such substance as sulphuretted oil, or turpentine, has been found particularly necessary; and owing to this, perhaps less benefit has been derived from them, than the practitioner has expected. I trust therefore an account of a machine, contrived to produce the effect very speedily, and with little labour, may not be unacceptable to many of your readers.

Combination of mercury with fats or mucilages tedious.

Means of accelerating it injurious.

Hence mercurial plasters perhaps defective.

Machine for the purpose.

The apparatus consists of a piece of cast iron, A, Plate IX. Fig. 1. about two feet long and four inches wide, curved so as to form a segment of a circle of four feet radius.

Description of the apparatus.

Description of the apparatus.

Perpendicularly to each side of this segment is fixed an additional piece of iron, B, by screws or otherwise; and another piece at each end, C, D, inclining in the direction of the radius of the circle; so as to stand above it about four inches, and form a box or trough. This may be made to stand on legs, or be fixed securely in any simple framing, at a height most convenient for the person that works or attends the machine.

A wrought iron pallet, E, is to be fitted accurately into the box, reaching from one side to the other, allowing it only sufficient space to work easily backward and forward. The lower end of the pallet is to be made to fit the bottom of the box; but its lower edges must be rounded off considerably, so as to rise over the matter in the box, and not drive it all before it. Its shape is more particularly seen at Figs. 2 and 3, which are on a somewhat larger scale, Fig. 2 being the front, and Fig 3 the side view of it.

This pallet is to be affixed to the end of a vertical shaft or rod, F, measuring four feet from the extremity of the pallet to the pivot, G, on which it turns. The top of the rod may be secured by working through a chaff mortice, which will allow it to move backward and forward, but not admit any lateral motion.

The piece, H I, in which this mortice is made, is fixed to a cross piece between the uprights, K, L, Fig. 5; one of which only, K K, is seen in this view: and as this cross-piece moves on a pivot at each end, though it is prevented from moving horizontally, it is confined vertically only by a weight at the extremity; which weight may be greater or less, according to the degree of pressure or friction required. Instead of the weight, a wooden or other spring might be made to act on the head of the shaft at G; but in general a weight will be found preferable.

The pallet is set in motion by means of a rod M; one extremity of which is attached to the vertical shaft at F, where it works on a pin; the other by coupling brasses to the crank, N, in the axis of a fly wheel, O. A perpendicular view of these parts, with the same letters of reference, is given at Fig. 5. The place where the rod, M, is attached to the vertical shaft, F, must be so proportioned

to

to the throw of the crank, that at every revolution of the wheel the pallet shall move backward and forward through the whole extent of the box; to which a cover may be fitted, with a longitudinal aperture sufficient for the shaft, as shown at Fig. 4.

Mode in which it acts.

The mercury, and the composition with which it is intended to be mixed, being placed in the box or trough, half on one side of the pallet standing in the middle, and half on the other, the fly wheel is to be turned by its handle, P, as in common operations. As long as the mercury remains in a fluid state, by its gravity it will follow the pallet to the centre of the box; and as some portion will mix with the composition at every turn, the whole will soon be completely blended together.

Particularly adapted to mercurial plasters.

This apparatus is particularly adapted for combining mercury with a composition of sufficient tenacity to form a plaster, which cannot be done directly in the common way; so that it is necessary first to subdue the quicksilver with turpentine, or sulphuretted oil, and then to mix it with a plaster previously melted. But with this apparatus the plaster is softened by the heat generated by the friction, and the power is sufficient to mix the mercury with it directly; and this both intimately and speedily.

Applicable to various other purposes.

It is almost superfluous to say, that the apparatus need by no means be confined to the dimensions here given; and by enlarging them it may be adapted to various useful purposes, which will readily suggest themselves to you. Where this is done, however, it might be found necessary to steady the horizontal piece, H, I, by allowing its extremity H to work between two uprights.

I am,

SIR,

Your obedient humble Servant,

W. X.

July 20, 1807.

II.

A Memoir on Two new Classes of Galvanic Conductors, by Mr. ERMAN.

(Concluded from p. 249.)

SECT. II. *Of Conductors, that, in establishing a Contact between the Two Poles, insulate the positive Effect, while they continue to propagate the negative Electricity*

Substances that act as conductors to either pole separately, but only to the negative, when they are in conjunction.

I HAVE placed in a fifth class those substances, which, applied to either pole separately, act as excellent conductors, but which, interposed between both poles, insulate the positive effect, without discontinuing to be perfect conductors of the negative. A wish to realise all the combinations possible in closing the galvanic circle, excited me to examine a great number of substances, in order to find some one that should come under this description. My attempts were long in vain, because the analogies that guided my research were very imperfect; and I did not discover the property in question in the flame of phosphorus, before I had found it unquestionably to exist in a solid body. This body is alkaline soap of every kind, provided it be in the highest state of dryness possible: at least I have found no perceptible difference in the electric effects, whether it were composed of vegetable oil or animal fat, converted into soap by pure soda, or soda mixed with potash, and prepared for pharmaceutical purposes, or the uses of the arts and domestic economy. All these soaps exhibited the effects I am about to describe, provided the essential condition of their being desiccated as much as possible were fulfilled.

Flame of phosphorus and soap

Hard soap, perfectly dry, applied to either pole, is a conductor.

A prism of hard soap, completely dried, and applied to either of the poles of a galvanic pile, conducts all the electricity of that pole into the ground, and produces a maximum of electric intensity at the opposite pole. In this respect there is no difference between the two poles, and the soap acts as the most perfect conductor would do. Of this I convinced myself by measuring with Volta's electrometer the divergence produced at each of the poles by the contact of

of metal, a wet finger, soap wetted at the point of contact, and soap perfectly dry; and I found them all equal in degree. It will soon appear why, notwithstanding this, no shock is obtained on employing a prism of soap, unless it be wetted at the point of contact.

If a wire from each pole communicate with an insulated piece of soap, the circuit will not be completed.

If now two wires, issuing from the two poles of the pile, have their extremities fixed in a perfectly insulated prism of soap, into which they should penetrate a few lines, no remarkable effect is perceived: that is to say, after having brought the two poles to the same intensity, by applying to them an insulated metallic rod, the electrometers of the two poles will act as they did before the intervention of the soap, and when a stratum of air insulated them perfectly with respect to each other. But the instant a free communication is established between the soap and the ground, the positive electrometer exhibits a maximum of divergence, and that of the negative side loses all signs of it, precisely as if a communication had been established between the ground and the negative pole itself. Consequently the soap, which insulates the positive effect, is a perfect conductor for the negative; to which it belongs throughout its whole extent, for if you touch the soap with a fine point ever so near the place into which the positive wire is inserted, it is impossible to take from it any portion of electricity, so perfect is the insulation of this pole.

If this soap be uninsulated, the negative electricity will be conducted off, but not the positive.

Touching the positive wire with a dry finger and the soap with wet does not form a communication if both fingers be wet a shock is felt.

A very striking proof of this paradoxical property is, if one finger be applied to the wire of the positive pole, and another finger wetted to the soap, no shock is felt, and the electrometers do not show the least change in their respective divergencies. But if the experiment be repeated by establishing a communication between the positive pole and the soap with both fingers wetted, a very perceptible shock will be felt, and the two electrometers will arrive at an equal and a very weak degree of intensity.

These facts are sufficient to establish the existence of this fifth class of substances; but on pursuing our researches farther we meet with many interesting phenomena.

An apparatus for decomposing water forming part of the positive

To discern these the better, the continuity of one of the wires should be interrupted, and an apparatus for extricating gasses be interposed between its parts. In this case no

hemical

chain is not affected, even if a wet conductor be in contact with the soap and negative wire.

But the circle is completed by the least wet between the soap and positive wire.

chemical effect will be perceived, the insulation of the positive wire being an insurmountable obstacle to it. If now a little sponge, or a piece of cloth, be wetted with water, and placed in contact both with the negative wire and the soap, every thing will remain as before, and there will be no trace of chemical decomposition. But the moment this wet conductor is so placed, as to touch at the same time the positive wire and the soap, gas will be extricated in torrents, and the electrometers will indicate the completion of the galvanic circle. Thus the smallest quantity of water is sufficient, to destroy at once the anomaly of insulation, which characterizes this substance, and convert it wholly into an excellent conductor. I have frequently seen this effect result from the simple application of a piece of money, which I had damped on one side merely by breathing on it, and which I afterward placed on the surface of the soap, and in contact with the positive wire; while the same piece of metal, in the very same position, produced no effect in its usual state of dryness.

A coin damped on one side.

No fluid but water produces this effect;

I know no fact, where the indispensable necessity of water in a galvanic action declares itself in a more astonishing way: for the property of converting the whole mass of soap into a perfect conductor for the two poles in communication, by the contact of the positive wire, belongs exclusively to water, and is not, as might perhaps be supposed, a property of fluidity in general. Mercury, naphtha, oils of every kind, and other liquors not aqueous, poured into a hollow made in the soap at the spot where the positive wire is inserted, produce not the least effect.

and it is decomposed in the process;

It is very remarkable too, that water thus applied between the positive wire and the soap undergoes the same chemical decomposition as in the apparatus for decomposing it. In fact, according to the nature of the metallic wire, with which the water or wet conductor is in contact, either an oxide will be produced in abundance, or a gas, which is easily discernible by the froth it occasions. Hence it is, that the time during which the interposed water produces its effect is always limited, being in the direct ratio of the quantity employed, and the inverse ratio of the intensity of the pile: but in all cases both the electrometrical and

so that the effect ceases when all the water is decomposed.

chemical

chemical effects, which depend on the presence of water, continue decreasing, and soon cease entirely, when all the water at the points of contact is decomposed. From that moment the soap resumes its characteristic property, and insulates the positive electricity.

If there be any moisture in the soap therefore, the operator will be deceived at first, the soap acting as a perfect conductor.

It will be proper to introduce here an observation of some importance to the success of experiments of this kind. They who would repeat them without being able to procure prisms of soap exposed to the air for some years, or completely dried by the action of an oven or a stove cautiously conducted, might be tempted to accuse me at first of not having seen clearly; for a communication being established between the polar wires by soap yet damp, both the electrometers and the apparatus for decomposing water will begin by indicating a more or less perfect completion of the galvanic circle. But the part that water acts in these phenomena perfectly explains this want of success. It is the portion of free water, interposed in the damp soap between it and the positive wire, that in this case conceals the characteristic property, by which this substance belongs to the fifth class. To evince this nothing more is necessary, than to suffer a few moments to elapse: the water foreign to the conditions of the experiment will be consumed with more or less rapidity, according to its abundance and the energy of the pile; and then the whole of the soap will insulate the positive electricity, while it will serve as a conductor to the negative. On taking out the positive wire, that has thus been inserted into damp soap, the point will be found oxided, if the metal be of a nature to admit it, which never takes place in soap perfectly dry. It is scarcely necessary to add, that, if this wire be cleaned, and inserted into any other part of the damp soap, the same exception to the general rule will again recur, since in this new point of contact the conductor will find a fresh portion of free water. The oxidation of the positive wire in damp soap might lead to the supposition, that the insulation of the positive pole is owing to the production of this non-conducting coat. But the contrary may be proved by employing platina wires, which exhibit the phenomena in question, as soon as the water interposed by chance or design

But this effect ceases by waiting a little, till the water in contact with the wire is decomposed.

Though the soap will act as a conductor again, if the wire be inserted in a fresh part.

The cessation of the effect is not owing to oxidation of the wire.

as in fact the electrometers and the apparatus for decomposing water show. If now two gold leaf electrometers be placed in contact with the two extreme portions of the wet conductor, we shall find, as long as the circle continues effectively completed, these two electrometers will exhibit opposite divergences; for that which is nearest the soap will diverge negatively, while that nearest the pile will diverge positively. This may be proved by touching that part of the string nearest the soap, by which the electrometer contiguous to it will be discharged, and the charge of the other electrometer will be considerably augmented; but the reverse will take place, if the part of the wet string nearest the pile be touched. The partition of electricity into two opposite zones therefore is beyond a doubt.

the end of the string next the soap will exhibit negative electricity, the other end positive.

Now let the positive side communicate with the soap by means of a wire, and let the wet conductor be interposed between the soap and the negative side, the circle will not be completed, and no chemical effect will take place, as has already been observed. But neither will the string exhibit any partition into electric zones: for if two electrometers be applied to the two opposite ends of this string, they will both diverge in the same direction, and in the same manner; and by touching the string in any part both will be deprived of their divergence. Now to destroy this homogeneousness of electrification, and communicate to the string the most decided polarity, it is sufficient to apply a wet conductor between the soap and the wire of the positive pole; for the moment its interposition has completed the circle, the two electrometers at the ends of the string will diverge in opposite directions; by discharging one the other will be charged; and this partition of opposite electricities will remain, as long as the apparatus for decomposing water continues to indicate, that the circle is complete. This completion of the circle and partition of the zones will cease at once, if the wet conductor applied to the wire of the positive pole be removed. I could wish, that other natural philosophers might be struck like me with the singularity of this accordance of effects, the importance of which I think I foresee, if it be farther pursued.

If the wet string be applied between the soap and the negative side this partition of electricities will not take place,

till a wet conductor has been placed between the soap and the positive wire:

and this effect will cease, as soon as the latter conductor is removed.

Among the numerous combinations I have tried, to as-

Soap will serve as a connecting

certain with some precision the particulars of the phenomena, that are afforded by soap applied to the galvanic pile, I have observed nothing, that is not completely explained by the definition of conductors of the fifth class. Thus a prism of soap applied to the positive and negative extremities of the two piles, each of which has the same number of plates but in an inverted order, connects these piles completely, as long as the object is not to complete the galvanic circle; and give to their poles the same divergences, as if they were connected by a perfect conductor. But to obtain the physiological or chemical effects, that require the completion of the galvanic circle, a wet conductor must be placed between the prism of soap and the positive pole. If this interposition were made at the negative pole, it would have no effect. The reason why I mention this experiment, which is only a corollary from what has been already said, is to point out a very direct solution of a point of theory, on which philosophers differ, that may be drawn from it.

medium between two piles, but not for completing the circuit,

without a wet conductor to the positive pole.

It has been asked, what kind of electricity belongs exclusively to each of the two different metals of the pile; and opinions have been divided on the point. They who assert, that the elements of the galvanic pile are silver, a wet conductor, and zinc, ascribe the positive electricity to the silver. They on the contrary who maintain, that the proper combination is silver, zinc, and a wet conductor, consider the zinc as the metal charged with positive electricity. I have hesitated some time between the two parties, for want of a direct unequivocal proof, and from unwillingness to sacrifice my scruples to the authority of Volta himself. Now it appears, that the properties of conductors of the fourth and fifth classes furnish the most direct and palpable means of deciding the question. Among several other analogous proofs, the following is one of the most evident, and most easy to be exhibited.

This leads to a solution of the question, whether the silver or the zinc be in the positive state.

Between the last pair of plates of zinc and silver in any pile, place, a slice of perfectly dry soap, then establish a communication between the two poles by the interposition of an apparatus for decomposing water, and no chemical effect will be produced. With a camel-hair pencil lightly moisten

A piece of dry soap being placed between the last two plates, no effect is produced:

if the side next the silver be wetted, it is the same: if the side next the zinc be wetted the pile acts effectually. The zinc therefore is in the positive state.

moisten that surface of the soap, which is in contact with the silver, and again establish a communication between the two poles: still it will be the same. But the moment that the soap is moistened on the side that touches the zinc, the chemical and physiological effects will exhibit themselves fully. Now as we have already found, that a wet conductor is efficacious between the soap and the positive side exclusively, it is demonstrated beyond controversy, that it is the zinc, and not the silver, which constitutes the positive agent in the pile.

I know not at present what other substances belong to our fifth class. It appeared above, that the flame of phosphorus must decidedly be referred to it. Frequently I have seen indications of the same property in animal jelly reduced to a certain degree of dryness, as well as in ivory: but other masses of these substances exhibited these phenomena in a very equivocal manner, so that I refrain from deciding upon them, and at present shall only mention soap and the flame of phosphorus as included in this class.

Animal jelly and ivory have sometimes appeared to belong to this fifth class.

Many supposed nonconductors might be examined.

It would be interesting to examine, with a view to this classification, a great number of substances, which have been considered as nonconductors, because the galvanic circuit is not completed by their interposition: but it is now completely proved, that this test is insufficient; and the argument in favour of the nonidentity of galvanism and electricity, taken from the mode of action of flame, shows that errours of this kind may prove dangerous to the theory.

Why is ice a nonconductor, water a bipolar conductor, and vapour again a nonconductor?

The field of observation here opened may prove fertile in general results for the chemistry of electricity. By what mechanism of action is it, that water, so far divested of caloric as to become solid, perfectly insulates the effects of galvanism, as I have elsewhere shown: that afterward impregnated with a certain quantity of caloric in the liquid state, it transmits these effects with certain modifications, dividing itself into two zones, one of which is a conductor of positive, the other of negative electricity: and that lastly this same water, in passing to the elastic state by an excess of caloric, returns again to the class of perfect nonconductors, as may easily be proved, by receiving

ceiving between the two polar wires of a pile, furnished with its electrometer, the current of vapour from an eolipile near the orifice, where it has its whole transparency, and is free from all mixture of vesicular vapour and precipitated water? When by a well-managed heat thoroughly dried soap is brought to a considerable degree of softness, this substance likewise undergoes a gradual change in its faculty of conducting the electricity of the pile; and the nearer it approaches a state of liquefaction, the more it loses the property of insulating the positive electricity in completing the circle between the two poles, so that ultimately we perceive evident traces of the decomposition of water in the interposed apparatus. Other substances lead to chemico-physical researches not less interesting. Sulphur is a non-conductor; so is its flame. Phosphorus and amber are both nonconductors; but their flames are conductors. Here is one anomaly. But how again are we to account for the difference in these two conducting flames? Why, in closing the circle between the two poles, does that of phosphorus insulate the negative effect, and that of amber the positive?

Soap when melted is a perfect conductor.

Sulphur and its flame conduct. Phosphorus and amber do not, yet their flames do, but opposite electricities.

It is very probable, that all these varieties of action are intimately connected with the chemical affinities of the two elements of the electric fluid; and we may flatter ourselves with the hope of some day obtaining results of importance, by sedulously varying and analyzing these facts. Lest however I should be accused of exaggerating the importance of these phenomena, in deferring their explanation, by way of concluding I will mention some hypotheses, which have formerly guided my researches, but which no longer appear plausible to me, since the facts that have presented themselves to me have become more numerous and diversified. I relate these only to show, that I have sincerely endeavoured to lay open the whole subject, so as to reduce it to a simple "is this all?"

Perhaps the phenomena may be traced to the chemical affinities of two electric fluids.

Do conducting flames, which in completing the circle insulate the negative effect, owe this property to a stratum of oil, which, formed of its elementary principles in the act of combustion, and deposited on the negative wire, renders it impermeable to the electric fluid? Carbon, hidrogen, and oxigen, exist in fact in most substances, which by their combustion

Hypothesis that oil is deposited on the negative wire.

combustion exhibit the phenomenon of negative insulation. It is natural too, that this oil-forming combination should be produced at the negative or hidrogenating pole, and not at the positive, where, on account of the oxidation that takes place, water and carbonic acid must rather be formed. Do not the fuliginous ramifications, that expand much more abundantly from the negative pole, owe their exirtence to this oleification, which detains them, renders them more compact, and feeds them by a continually renewed combustion; while on the positive side the more perfect oxidation causes them to disappear in gas and vapour, before they have been able to expand themselves?

Reasons why this cannot be the cause.

This specious hypothesis involves the following difficulties. The flame of the purest hidrogen gas insulates the negative effect. Now where shall we find in this the carbon necessary for the formation of oil? On inspecting a thousand times, even with a microscope, the negative and positive wires perfectly cleaned, and kept a long time in the flame of alcohol, I never could perceive the least difference between their extremities. Besides, on bringing together with the greatest possible dexterity the positive and negative wires in the flame itself, a spark is constantly perceptible. Farther, both the electrometers and apparatus for decomposing water show, that, the moment any filaments of the arborescent soot extend from one wire to the other, the galvanic circuit is completed; which would be impossible, if the negative wire were rendered impermeable to the electric fluid by any non conducting coating. Lastly, how can it be supposed, that such an insulating coat should be formed in a single instant over all the surface of a disk of several inches, held two feet above the flame? The cause of the phenomenon then, which all these facts have placed before us, is not so superficial, as the hypothesis supposes.

Hypothesis, that acid generated at the positive pole abstracts the alkali of the soap, and then leaves a coat of oil.

The following is an analogous hypothesis, which likewise I had formed respecting the mode of action of soap in insulating the positive effects. The positive wire of the pile is the seat of oxigenation, as the negative is of hidrogenation. If then the alkali of the soap be neutralized by the contact of the acidifying wire, the oil, or fat, will be set at liberty, and thus insulate the positive pole, the conductor of which

it

it surrounds. Indeed I have found, that, in an alcoholic solution of soap diluted with water, a manifest separation of the oleaginous base of the soap will be effected after some hours, and it will be deposited on the wire of the positive pole. In fact oil is set free.

This fact is certain: yet it is easy to show, that the hypothesis to which it serves as a base is not less manifestly in contradiction with several particulars of the phenomena, which it ought to explain. In reality, when several prisms of soap, connected together by intermediate arcs, are exposed to the action of the pile, there is no doubt a partial insulation with respect to each point of insertion that corresponds with the positive effect: but we cannot thence conclude, that this insulation is absolute, since the negative pole may be acted upon through all these prisms, and all the points of insertion of their conducting arcs, so as to take from it its charge. At the points of contact of the positive wires therefore there is no absolute obstacle to the passage of the electric fluid, and the hypothesis of an insulating coat of oil falls to the ground. Besides, in perfectly dry soap the positive insulating effect displays itself the instant it is applied, when no preceding chemical decomposition can have taken place. Yet this cannot be the cause of the phenomena.

On this hypothesis too how we shall explain the production of the same effect by the flame of phosphorus? Must we recur to a new hypothesis to account for this single fact, and say for instance, that here the oxiding action of the acid in the state of vapour, being produced with more energy at the positive wire, renders it impermeable to the electric fluid? But this hypothesis would be equally untenable, since platina wires exhibit the phenomena of positive insulation as well as any other metal; and this effect manifests itself the first moment of contact exactly in the same degree as after the long-continued action of phosphorus in ignition. Besides, on this supposition it would be difficult to explain, why sulphur does not produce the same effect. Farther difficulties in the way.

I am persuaded therefore, that these hypotheses are completely erroneous; that the cause of the phenomena we have discussed lies deeper, and is purely chemical; and that we The cause therefore remains to be shown.

we shall not be able to explain it, till these facts have been more thoroughly studied, than has yet been in my power.

Classification of substances with regard to electricity.

Meantime I would propose, for convenience, the following classification and nomenclature. All substances, applied to the poles of the pile are either, Class 1, *insulators*; or they are conductors. The latter are distinguishable into, Class 2, *perfect conductors*; and imperfect conductors. The imperfect are, Class 3, *bipolar imperfect conductors*: Class 4, *positive unipolar*: and Class 5, *negative unipolar*.

III.

Inquiries concerning the Oxidations of Iron; by Mr. Dalso.

(Concluded from p. 280.)

Farther differences in the oxides of iron by sulphuretted hidrogen.

I DISSOLVED six grains of iron in muriatic acid without heat; and at the same time, in a separate vessel, six grains of red oxide, which I saturated with sulphuretted hidrogen. Four hours after I precipitated both these solutions by an alkali, and I found, that the precipitates of the green solution by sulphuretted hidrogen passed to red with the greatest rapidity. On pouring off the supernatant fluid, and letting water fall from some height on the oxide, it turned red immediately; but the precipitates of the other solution resist this trial. The green oxide by sulphuretted hidrogen, redissolved in muriatic acid, precipitates red; or at least it does so after two solutions. The common green oxides of iron, when recent, retain their colour even after being redissolved in acids five or six times. The reason of this no doubt is, that in the common green solutions of iron the hidrogen combines with the iron in the state of nascent gas, or very dense, and forms a more solid combination, than that into which the hidrogen furnished by sulphuretted hidrogen enters with the red oxide.

The green oxides hidrurets. This explains their alteration in the air.

If the green oxides of iron be hidrurets, as I suppose, it is easy to account for the alteration, that the green salts of iron undergo by exposure to the air. It is not to be wondered at, that hidrogen combined with oxide of iron should be volatilized spontaneously at a heat above 10° [54½° F.].

Almost

Almost all the combinations into which hidrogen enters are decomposed in the same manner, particularly when they are dissolved in water; as sulphuretted, phosphuretted, and carburetted hidrogen. All the vegetable acids likewise are decomposed spontaneously, when they are dissolved in water: and alcohol diluted in water is the same. The atmospheric air has no more influence in these phenomena, than it has in those of fermentation and putrefaction. All these operations require open vessels, because they evolve different gasses, which, if they were confined by any pressure whatever, would check the progress of the operation.

The experiments related in this paper I consider only as the outlines of a more extensive and deeper investigation; but as different circumstances have already obliged me to defer this research for one twelvemonth, and it is very doubtful how much longer it may be, before I shall be able to enter upon it, I was desirous of announcing these facts to the chemical world.

These experiments lead to farther inquiry.

Corollaries deducible from the preceding Facts.

1. All the oxides of iron soluble in acids are red: and though their proportion of oxigen varies from 15 per cent to more than 50, they are not distinguishable from each other by any means hitherto employed in chemistry.

2. The white oxide of iron is a salt with excess of oxide.

3. The green oxide is not a peculiar oxide, but a hidruret, or a combination of the red oxide with hidrogen.

4. The atmospheric air has no influence on solutions of iron, at least in the ordinary temperature of the atmosphere.

5. The saturation of iron with oxigen in its oxides does not destroy its magnetism, as hitherto has been asserted. Every oxide of iron is magnetic, or may become so without losing an atom of oxigen.

Inferences from them.

Note, referred to, p. 224. It has long been observed, that the magnetism of iron is weakened or disappears altogether in its oxides. At different periods this phenomenon has

Magnetism of iron diminished or destroyed in its oxides.

Supposed causes of this.

has been differently explained, according to the manner in which metallic calces were considered. Previous to the pneumatic theory, the magnetism was ascribed to the presence of phlogiston. After the labours of Lavoisier had shewn, that the formation of metallic calces was owing to the combination of oxigen with the metal, chemists naturally inferred, that the oxigen destroyed the magnetism: and as on the other hand facts seemed to prove, that magnetism was annihilated in oxides highly loaded with oxigen, it was established as a principle, that oxides of iron at a maximum, or red oxides, were not magnetic.

Loss of phlogiston.

Oxigenation.

This inconsistent with some facts.

Oxide at a maximum magnetic.

This principle, which does not agree with the fact I have just related, embarrassed several philosophers in explaining certain phenomena. The celebrated Baron von Humboldt, who discovered magnetic polarity in a serpentine, could not account for this property in a mineral, which appeared on analysis to contain only superoxigenated oxide. On this occasion Guyton observed, that the term of superoxigenated, employed by the Baron, was inaccurate, for these two properties of being magnetic and superoxigenated were incompatible; and that the magnetism of the Saxon serpentine, and of other minerals which do not afford green oxide by analysis, should lead us to suppose intermediate oxidations of iron. Mr. Haüy, the learned natural philosopher, to whom magnetism is indebted for very perspicuous elucidations, has likewise suffered himself to be led away by the chemists; and, endeavouring to account for the magnetism, which some red oxides of iron acquire when strongly heated, says, that "this is owing to "the heat reducing some particles of the oxide, at the same "time that it assists the magnetic action of the globe, &c."

This denied by Guyton.

Haüy's mode of accounting for the restoration of magnetism to an oxide by heat.

At first I subscribed to such respectable authorities, because, as Bacon observes, *oportet ediscentem credere;* and because, in the commencement of these researches, I tried several red oxides, obtained from different solutions of iron, as well as several aperitive saffrons of steel, which did not give me the least sign of magnetism. But as I afterward perceived, that oxides greatly loaded with oxigen, or such as contained ·50 or ·56, retained their magnetism, while others

Magnetic oxides with much oxigen.

others that had scarcely 20 * were not attractable, I concluded, that there was some other cause acting at the same time with the oxigen, or perhaps exclusively, to destroy the magnetism. Reflecting on the circumstances, that preside over the formation of all these different oxides, I suspect, that in these phenomena, as in most of those to which the oxides of iron give birth, too much has been ascribed to the influence of oxigen, by referring to it effects in which it has no concern. If the loss of magnetism in some red oxides of iron be not exclusively owing to a state of extreme division, this at least has a more decided influence on it than the presence of oxigen.

Unmagnetic oxides with little.

The magnetism destroyed by division.

When the magnetic oxide, of which I have spoken, is precipitated by concentrated alkalis, and without the solution having been much diluted by water, the precipitate is a more or less blackish brown, it does not change by drying in the open air, and it is decidedly magnetic. If, on the contrary, the solution and the alkali be diluted with water, which has been boiled a long time to remove every suspicion of superoxidation, the precipitate is red, like all those called oxides at a maximum; and if it be dried in the open air, or by a gentle heat, like the preceding, it gives no signs of magnetism. Now we cannot ascribe this difference of colour and of magnetism to a different proportion of oxigen; for if we try the experiment with two equal parts of oxide, we shall find, that the weight of the red oxide is the same as that of the magnetic. The difference of magnetism therefore, like that of colour, depends on the difference of density, or the greater or less distance between the particles of the two precipitates.

Concentrated solutions throw down magnetic oxides:

dilute solutions do not.

In fact, when the solution is concentrated, the particles of the oxide touch each other, or at least are much nearer together, than when the solution is diluted with water: and this difference of approximation is in the ratio of the bulk of the two solutions, since the distribution of the oxide in both cases is uniform. Let us suppose, that the difference of approximation be in the ratio of one to ten; or,

This is owing to the state of approximation of the particles, which is in the inverse ratio of the quantity of the menstruum.

* Those obtained from the green solutions, of which I have spoken in the course of this Paper. See p. 273.

which amounts to the same thing, that the thickness of the columns of fluid separating the particles is $\frac{1}{10}$ of a line in the concentrated solution, and a line in that diluted with water; what will happen, if a drop of alkali fall on any point of the concentrated solution? The alkali will determine the precipitation of a certain number of particles of oxide, which will be at first $\frac{1}{10}$ of a line from each other, as when they were combined with the acid: but their specific gravity, assisted by the pressure of the atmosphere and of the solution, will be capable of overcoming the resistance opposed to their approximation by the little column of liquid that separates them. This is the reason why the precipitate is blackish, retains its magnetism, and at the expiration of a few minutes is insoluble in cold muriatic acid.

In the solution greatly diluted with water, though the alkali determines the precipitation of an equal number of particles, and though their specific gravity and pressure act in the same manner, as the resistance opposed to them by the columns of fluid is ten times as great, their approximation cannot be so complete. Hence the difference of colour, absence of magnetism, and facility of solution in acids.

The case is the same with oxides by calcination,

Besides, when extremely fine filings of iron are calcined, and divided by trituration in the course of the operation, till they have taken up 15 per cent of oxigen, we obtain a very fine red powder, much less magnetic than oxides with 30 or 40 per cent, obtained by the common process; that is to say, with common iron filings not triturated during the course of the process.

and red oxides when rendered magnetic by heat.

Finally, the red precipitates of solutions of iron, and most of the aperitive saffrons, after they have been well dried, exhibit no signs of magnetism. But if they be exposed to a strong fire for some time, their bulk diminishes, their colour is heightened, and they are decidedly magnetic. Now we cannot say here, that the magnetism is owing to a loss of oxigen, since the experiments of Proust, and more recently those of Berthollet, have proved that these oxides, exposed to the strongest heat of our furnaces, do not give out an atom of oxigen. To the same approximation is to

be

be ascribed the conversion of red crayons into magnets, related by Haüy in his elementary Treatise on Natural Philosophy; and the magnetic polarity, that displays itself in all the oxides of iron heated before the blowpipe, observed by Mr. Lelièvre.

That magnetism must be weakened by minute division of the substance deducted from the theory.

Besides the weakening of magnetism by division, and even the complete suspension of its effects, are consequences of our theory of magnetism. Though I am not acquainted with any accurate experiments, which prove, that magnetism acts in the direct ratio of masses, a number of facts attest, that it is subjected to this law*. Every one knows, that under similar circumstances a magnet eight inches long and an inch thick is more powerful than another of half these dimensions. The two hypotheses, that account for the magnetism of the earth, rest likewise on this law: for it is in consequence of the magnitude of their mass, that the action of the magnetic nucleus, or of mines of iron, extends to such prodigious distances. Without supposing this law, we cannot account for this phenomenon. Thus, all other circumstances being equal, a grain of iron will have a hundred times the magnetic power of $\frac{1}{100}$ of a grain, a thousand times that of $\frac{1}{1000}$ of a grain, and so on: and the imagination can easily conceive a subdivision, by which the magnetic power of a grain of iron would be so divided, and its sphere of action so shortened, that the magnetism of each particle should not only be unable to pervade the space that separates it from another, but even to exhibit any signs of magnetism, when brought into contact with a magnet. An example will elucidate this.

This illustrated.

Suppose I present the north pole of a needle to a particle of iron filings. The austral fluid of this particle will place itself at the extremity nearest the needle, while its boreal fluid will be expelled to the opposite extremity. But as there is a sufficiently appreciable difference between the distance at which the north pole of the needle acts on the two fluids of the particle of iron, the south energy of this will overcome the north, and through this preponderance it will approach the needle.

* And even though this law should be slightly modified in some way, the effects of this modification would be of little account in the present discussion.

Now let us continue to subdivide this particle, till the distance between the two poles of its molecules, brought in contact with the needle, shall be so small, that the action of the two poles shall be as we may say compounded together: the difference between the attraction and repulsion will then become inappreciable, and the molecule will give no sign of magnetism.

Restoration of magnetism to an oxide not the effect of calorie,

It may be said, that this development of magnetism in oxides strongly heated is rather owing to the action of the heat weakening the coercive power that opposes the magnetism. But beside that this coercive power is not a fact so certain as the approximation, that these oxides undergo whenever they become magnetic, it can account only for part of the phenomena, since in oxides by precipitation, which can be obtained magnetic at pleasure, heat has no concern.

for it takes place without heat.

Whether oxigen may weaken magnetism is not determined.

For the rest, whatever be the cause of this phenomenon, it is proved, that oxides saturated with oxigen are magnetic, or at least may become so without losing an atom of oxigen. I do not however mean to assert, that a given quantity of iron saturated with oxigen retains the same magnetic power as it possessed before it was oxigenized; for on this subject I have made no experiments.

IV.

Description of a Curvilinear Saw, invented by JOHN TROTTER, *Esq., of Soho Square, from whom the following Communication was received*.*

GENTLEMEN,

A curvilinear saw very desirable

WITH the view of obviating many difficulties and expenses, which have long attended the operations of those requiring curvilinear sawing in their trade, and of public bodies connected with those trades, through the licentious and refractory conduct of sawyers, it has been represented to me as a measure extremely desirable, to adopt more ge-

* From the Transactions of the Society of Arts, &c. for 1806, who voted their gold medal to Mr. Trotter for this invention.

nerally

nerally mechanical powers, could such be discovered as would preclude much mystery and manual labour.

Considering the subject in a national point of view, as connected with our naval yards in the formation of timber; with our military departments, in respect to wheels of every description; with our whale and herring fisheries; our public and private breweries and distilleries; our East and West India Companies, and other bodies depending on cooperages, as well as other minor trades peculiarly liable to the evils complained of; I invented a curvilinear saw, which, with little aid of the most ignorant labourer, answers every purpose. on several national accounts.

Having effected these ends, suffer me to solicit the honour of your acceptance of a model, together with a drawing of my saw, sufficiently accurate for the use of those in remote situations to work by, who may wish to use or make them.

I have the honour to be,
Gentlemen,
Your most obedient and most humble Servant,
JOHN TROTTER.

Soho Square, Sept. 12, 1805.

To the Society for the Encouragement
of Arts, &c.

Reference to the Engraving of Mr. Trotter's Curvilinear Saw. Plate X.

Fig. 1. Represents a bird's eye view of the saw and machinery. The dotted lines show the spindle *a*, moving on two centres *b*, *b*, having at one end a pulley *c*, and at the other a concave saw *d* (with a corresponding convexity to the curve required to be sawed,) secured on the convex side by a collar, and on the concave side by a loose collar, and screw nut. Description of one.

e, *e*, Two grooved plates, admitting through the top of the bench and fence *f*, screw bolts fastened by thumb nuts, by means of which, and a parallel motion *g*, the fence *f* is regulated, and consequently the conductor *h* of the wood *i* admits it to be sawed through, as represented in the dotted line at any part required.

The fence, conductor, and saw, must all be curved alike; but to saw in smaller circles, with the same saw and at the same time square at the face of the bench, a steel slider *k*, regulated by two screws, is made to press, as occasion may require, on the convex side of the saw, and raise the vertical line of it to a right angle with the bench; otherwise the top of the bench itself must receive the same inclination to the vertical line of the fixed saw.

Fig. 2. Is a front view of the saw and bench, in which the teeth of the saw are more clearly shown.

Fig. 3. An end view of the same machinery.

Fig. 4. Shows the saw, axle, and pulley, all made of iron or steel, and separated from the frame.

V.

Account of a Bookbinder's Cutting Press, for which Fifteen Guineas were voted to Mr. JAMES HARDIE, *of Glasgow, by the Society of Arts* *.

SIR,

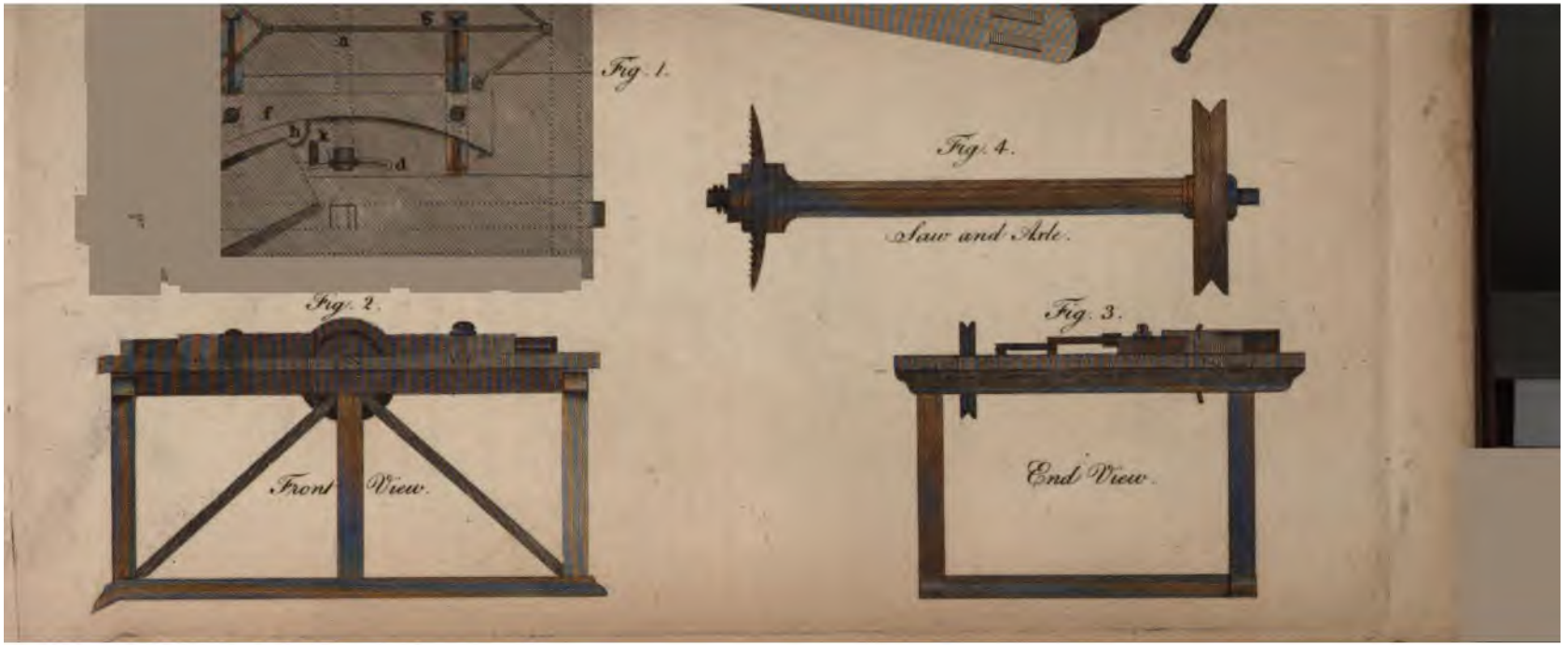
The press more powerful than the common, and saves time.

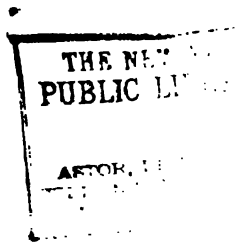
I HAVE herewith sent a model of an improved press for bookbinders, the invention of Mr. James Hardie, bookbinder, Glasgow. The inventor claims no other merit than that of having simplified the common press, rendered it more powerful, and adapted it to work more economically; or, in other words, to save time to the workman.

Generally used at Edinburgh and Glasgow.

It has been found so superior to the press in common use, that all the bookbinders in Glasgow and Edinburgh are adopting it. This is perhaps the best proof that can be given of its utility. The inventor has received certificates from the bookbinders alluded to, which will be sent to the Society, if they think the press worthy of their notice. Mr. Hardie, in desiring me to submit the model to the inspection of the Society, has in view chiefly to benefit the bookbinders in places remote from his residence, an object which he thinks cannot be so well attained in any other way, as by the publicity which the Society is able to give to improvements deserving of its notice.

* From their Transactions for 1806.


a
b
c
e
f
g
h
d
Fig. 1.
Fig. 4.
Saw and Axle.
Fig. 2.
Front View.
Fig. 3.
End View.


THE NE
PUBLIC LI
ASTOR,

The improvement of this simple instrument has cost Mr. Hardie much time, and even expense; and he will be glad to receive any remuneration from the Society which they may think his invention deserves.

I am, Sir,

Your most humble Servant,

A. TILLOCH.

To C. TAYLOR, M. D.

Twenty-three persons testified by their signatures and subscriptions to Mr. Hardie their approbation of his Cutting Press.

Reference to the Engravings of Mr. J. Hardie's Bookbinder's Cutting Press. Plate X. Fig. 5.

The principal difference between this and the press which has been from time immemorial employed by the bookbinders consists in effecting the business by one iron screw, instead of two wooden ones formerly used. This screw works in a nut let into and screwed to the top piece A, its lower end working in a collar, screwed to the moving piece B, sliding in grooves within the two sides of the frame. CC are the guides for the plough, as in the common press. Description of the press.

VI.

On Blende, and some other Articles; by PROFESSOR PROUST*.

THAT zinc is incapable of disputing oxigen with charcoal, is a known fact: and the same may be said of sulphur. Consequently, if blende contain oxigen, it must yield it to the action of charcoal. The zinc in blende not an oxide:

I kept a mixture of transparent yellow blende and fir charcoal at a red heat for an hour, but I did not find the slightest indication of sulphurous acid. The mixture being washed, to separate the charcoal, left the blende behind, which had undergone no change. Where then is the oxigen of blendes?

* Journal de Physique, Vol. LXIV. p. 150. Feb. 1807.

I have

and the metal in it saturated with sulphur.

I have heated redhot a hundred parts of the same blende with as much sulphur; and, when the operation was ended, it had not increased a single grain, or even changed colour. Hence we may conclude, first, that the metal in blende is saturated with sulphur; and secondly, that it is free from oxigen, otherwise the sulphur is a combustible, which would have taken its oxigen from it. The following experiment does not allow me to doubt this.

Artificial sulphuret of zinc,

I heated together a mixture of sulphur and pure oxide of zinc, a hundred and twenty-five grains of each: the produce was a hundred and thirty-six grains. Apprehensive however, that it might not be saturated, I heated it with fresh sulphur, by which it was increased to a hundred and thirty-eight grains. On heating it with sulphur a third time, it did not go beyond a hundred and thirty-eight. I repeated the experiment twice more, and the product stopped at a hundred and thirty-eight grains. Hence we may infer, if there were no mistake, that 38 parts of sulphur took the place of 25 of oxigen, that were condensed in the oxide. It would be superfluous to say, that this process evolved torrents of sulphurous gas.

38 parts sulphur to 100 of zinc.

Morveau is I believe the first, who reproduced sulphuret of zinc by heating its oxide with sulphur. The artificial blende remains pulverulent: but it appears to me by Morveau's account, that it is capable of being melted by a strong heat. Blende is transparent; hence, they say, its metal must be oxided. But the sulphurets of mercury and of arsenic are transparent likewise; yet they are free from oxigen. The sulphuret of arsenic, I know not whether I have mentioned the fact, supports any temperature to which you choose to expose it, without affording any indication of sulphurous gas, or losing its transparency. Arsenic acid or oxide gives out sulphurous gas in abundance, when heated with sulphur, and affords a transparent sulphuret, similar to that produced by the metal itself. These compounds therefore contain no oxigen; and consequently transparency is no argument for the oxidation of a sulphuret. But why should zinc refuse to unite directly with sulphur? I confess I see no reason for it. I had intended

Transparency of a sulphuret no proof that the metal is oxided. Sulphuret of arsenic.

to

to treat zinc with cinnabar, and other sulphurets; but different objects have prevented me.

The sulphuret of zinc is frequently concealed by foreign oxides and sulphurets: hence red, black, ash-coloured, green, and other blendes, of which so many species have been made. Now this is precisely the same, as if, in the natural history of wool, different species were to be made of those that are dyed red, black, gray, or green. Different colours do not constitute species in blendes.

There are blendes coloured by red oxide of iron, which appear black; but their powder is red. These may be analysed by muriatic acid, which will cause the iron to descend to its minimum of oxidation, on account of the sulphuretted hydrogen formed during their solution. We should be aware of this, that we may not suppose the oxide to be at a minimum, where nature has placed only oxide at a maximum. Black blendes coloured by red oxide of iron.

There are some that contain lead in the state of oxide, or of sulphuret. If these be exposed to the action of muriatic acid, the whole of the lead is found in the solution: but if oxigenized muriatic acid be used, we must look for the lead in the residuum only. The reason of this is obvious; the sulphur of the blende, being acidified, precipitates the lead in the state of sulphate. Blendes with lead.

Hidrosulphuret of Zinc.

Sulphuretted hidrogen precipitates zinc from its solutions in a yellowish white powder, which is a hidrosulphuret. This precipitation however is limited. When the acid is freed from a considerable portion of the oxide, so as to be in excess, it disputes the remainder with the hidrogen, and the precipitation stops. It is necessary therefore, to add a little potash, to neutralize this excess. The alkaline hidrosulphurets produce the same precipitate. The nitric acid acts with vehemence on this hidrosulphuret, burning its hidrogen, and part of its sulphur. Muriatic acid applied cold expels the sulphuretted hidrogen in abundance. This hidrosulphuret at a red heat gives out water and sulphurous acid, and is converted into a simple sulphuret, or blende. The sulphuret of zinc, whether native or artificial, yields sulphuretted hidrogen; which however is not an educt, but a product. Hidrosulphuret of zinc.

Ambergris.

Ambergris.

Yellow ambergris from Brasil.

This piece was found on the coast of Brasil. It is of the colour of honey; very homogeneous in its texture; and free from those fragments or beaks of the cuttlefish, that are interspersed in the ash-coloured amber of the shops. Alcohol dissolves it entirely, except a few slight pellicles. This solution is curdled by water. Evaporated it leaves a yellow substance, which softens and burns like a resin; swims on a solution of potash, which dissolves but a few atoms of it; and gives out no smell of ammonia. The fragrance of this purified resin is still that of the ambergris itself. Exposed to distillation it melts quietly, without swelling up, and yields a thick, yellow oil, which swims on water. It is accompanied with some indications of an acid: but what is astonishing is the ambergris scent of this oil.

Cochineal.

Cochineal acid. Its colouring matter precipitated with lime, but obtainable more pure with oxide of tin or lead.

The powder of this insect has always seemed to me to have an acid taste. I know not whether it be the effect of the action of air on any of its principles. Lime water precipitates its colouring matter completely, and the result is a lake, on which alcohol has no action. To obtain the pure colouring principle, we should decompose this lake: but as the white oxides of tin and of lead likewise saturate themselves with it readily, we may obtain it still more pure from these by means of sulphuretted hidrogen, than by employing acids. I believe the colouring principle of kermes likewise precipitates with lime.

Ox gall.

Resin of gall.

Acids precipitate from this a resin, which, after it has been well washed in boiling water, may be drawn into threads like boiled turpentine. When dry, it is semitransparent, greenish, melts with the gentlest heat, and on burning coals exhales a smoke that has somewhat of a fragrant smell. Alcohol dissolves it, without leaving any residuum; and water precipitates this solution. Oxigenized muriatic acid whitens it with the assistance of a gentle heat. It remains in complete fusion, after having given out some moisture:

oisture: and a strong heat causes it to rise in the form of thick oil, the smell of which is that of an animal substance, and unpleasant. A little carbonate of ammonia :compani**es** it. It leaves very little coal. But what dis- Soluble in nguishes it from the aromatic vegetable resins is its great alkalis. lubility in the weakest alkalis. Acids separate it from iese without alteration, and it may be drawn out into Not always the ireads as before; so that we cannot deny it the principal same. haracters of resins, but it is not always the same. I have btained some from gall, which was soft, semifluid, or inapable of assuming the consistency of the preceding.

To obtain this resin pure, we must begin with passing Mode of obried gall through alcohol, to separate its albuminous por- taining it pure, ions. These occasion the putrefaction of gall when kept: and preserving ut the extract, passed through alcohol, and evaporated to gall from pue consistence of a sirup, is no longer susceptible of alte- trefaction. ation; and it is in this state I keep it for my lectures*.

VII.

Remarks on the Structure of Mount Jura, from a considerable Number of Heights taken by the Barometer, and extended through France to the Sea; by Mr. And. de Gy, *Member of the Academy of Cassel, &c.*†

. THE four loftiest summits of the first chain of the Jura First chain of re nearly on a level, as is their base, the lake of Geneva, mount Jura. n a length of twelve leagues.

The summits of the Suchet and Sucheron, which rise from he western extremity of the lake of Neufchâtel, six leagues om the preceding, are 30 toises lower. The other sum-

* An ingenious artist, Mr. J. Clark, in his Instructions for Drawg and Painting in Water Colours, observes that gall, which it is equently necessary to add to a tint, when it will not adhere unirmly to the paper, from any slight greasiness of its surface, will eep much better, if it be boiled a little. This no doubt is owing the coagulation and separation of the albuminous matter by heat, nformably to the remark of Professor Proust. W. N.

† Journal des Mines Vol. XVIII. p. 430.

mits

mits continue diminishing to the Rhine; but to the Lebreberg, five leagues below Soleure, or through a space of 25 or 30 leagues, they decrease almost imperceptibly, as does their base. In this interval indeed the summit of the Chaserale is not above 40 toises lower than the four highest; but from the Lebreberg to the Rhine the declivity is more rapid.

The plains or table-lands, that form the western foot of this first chain of the Jura, likewise diminish in height proportionally to the mountains, and in the same direction.

Second chain. 2. The highest summits of the second chain of the Jura continue nearly on a level with each other as far as the Stierberg, opposite the Lebreberg, a distance of 20 or 25 leagues: but thence to the Rhine they also lower more rapidly, like those of the first chain.

Third chain. 3. The highest summits of the third chain are nearly on a level with each other throughout its whole length, which is 30 or 35 leagues.

Fourth chain. 4. The highest summits of the fourth chain, which are nearly in a right line from Estival, four leagues north of Moirans, to St. Hippolytus, a distance of 20 or 25 leagues, are likewise nearly on a level; but those to the south of Estival, and to the west of the above line, follow the inclination of the rivers.

There are several large plains that intersect this chain, particularly above Ornans and Villafans. These plains are nearly level throughout.

Fifth. 5. The fifth chain is very similar to the fourth.

Sixth, or lowest chain. 6. The lowest chain of the Jura is about 60 leagues in length. Its loftiest summit is that called the *haut des Tronchats*, three leagues east of Porentrui. From this point the other summits diminish on both sides, according to the course of the rivers. There is no great difference from the mountain of St. Ursane to the Mont de Triève, two leagues north-east of Beaume: they afterward diminish more, and remain on a level with each other to the mountain of Pouillat, three leagues north-east of Bourg-en-Bresse, that is for 35 or 40 leagues.

The plains at the eastern foot of the last summits are also nearly on a level with each other, particularly from Orgelet to Cernans, above Salins.

From

this arrangement it follows, that the whole of the
ms an amphitheatre of 60 leagues long, by 12 or 14
he steps of which, or chains of mountains, that
e one another in a line from north-east to south-
e level, each almost throughout its whole length.
the chain that separates the waters that fall into
seas, the loftiest summits, except the Haute-Joux
ette, are in the neighbourhood of Dijon. From
at, following the chain to the south-west, the
ummits are nearly on a level; but the other sum-
his chain decline to the north-east.
and Besançon are level with each other, and the
ummits in the neighbourhood of these two cities,
l by a plain of 14 or 15 leagues and a few hills,
wise on a level. The summit called Tasselot, the
ear Dijon, is 306 toises above the sea: the rocks
faucon, the highest near Besançon, are 303 toises
e sea. The other summits to the south-west of
d of Besançon, in the two chains in which these
situate, are likewise on a level in their other
nding points on each side of the Saone, at 12 or
s distance, and in a length of 20 leagues.
all the valleys were filled up to the height of the
of the mountains that border upon them, from the
e tops of the Alps, a very gently inclining plane
formed. For instance, if a right line were drawn
vre to the loftiest summits of the mountains, that
groupe of St. Gotthard, it would cut the inter-
chains of mountains nearly at right angles, passing
ues south of Paris, between Langres and Dijon,
ues north-east of Besançon, two leagues north of
tel, and a little to the west of Berne.
ne would be 160 of the old common French leagues
[445 miles], and the whole ascent would be only
ses [3733 yards], the height above the sea of the
erberg*, the loftiest summit of these mountains;
f the declivity were uniform, it would be a little

The whole forms an amphitheatre.

Chain that divides the rivers of France.

General level from Havre to the summit of the Alps.

* height here given is not from actual measurement, but from a comparison with them that were measured.

less

less than 11 toises per league. But, as the whole slope is not uniform, to give a more accurate idea of it, we may divide the 160 leagues into five unequal portions, each of which would have a nearly uniform slope.

Level from Havre to Dijon.

1st. From Havre to the chain that separates the waters falling into the two seas, taken near Dijon, a length of 95 leagues, with 300 toises elevation above the sea, which would give an ascent of three toises, one foot, four inches, in every league.

From Dijon to Besançon.

2d. From these summits to those of the lowest chain of the Jura, beyond Besançon, at both which places the height above the sea is the same, making a horizontal line of 23 leagues.

From Besançon to the top of the Jura.

3d. From the highest summits near Besançon to those of the highest chain of the Jura, at Casserale, two leagues north of Neufchâtel, a distance of 12 leagues, with 550 toises difference of elevation, giving $45\frac{10}{11}$ toises rise in a league.

From the Jura to the first chain of the Alps.

4th. From the loftiest summits of the highest chain of Jura to those of the lowest chain of the Alps, south-east of Berne, a distance of 15 leagues, with 200 toises difference of elevation, giving 13 toises two feet rise in a league.

From the lowest Alps to St. Gotthard.

5th. From the summits of the lowest chain of the Alps to the highest of those of St. Gothard, 15 leagues distance, and 700 toises of elevation, being a rise of 46 toises four feet in a league.

VIII.

Some Mineralogical and Geological Observations, made in the Isle of Arran; by the Rev. JAMES HEADRICK*.

ADVANCING along the north shore of the island from Loch Ransa, the first remarkable thing that occurs is at Craig-na-Srone *(Nose Rock,)* where the secondary strata are seen resting upon the micaceous schistus. The first

* From that Gentleman's View of the Mineralogy, Agriculture, Manufactures, and Fisheries, of the Island of Arran.

stratum

stratum of this kind is a sort of chalky limestone, which contains rounded pieces of quartz, fragments of schistus, &c., so as to constitute a species of pudding stone. Many parts of the stratum contain few of these extraneous bodies, and would make excellent lime. Further on, a sort of pudding stone, red sandstone, and shiver, occupy the coast. Calcareous breccia.

Where the limestone rests upon the schistus, three whinstone veins intersect the schistus, and the strata that rest upon it. Where these veins run in the schistus, the cheeks on each side are penetrated by the whinstone; and fragments of the schistus are found immersed in the whinstone veins. One vein divides into two, like the letter Y; and pieces of slate are found immersed in the whinstone, at the angle of separation. Schistus intersected with whinstone.

The Scriden rocks are strata of breccia, or puddingstone, of most enormous thickness, and leaning upon the side of the mountain at an angle of about 45°. About a hundred years ago, immense masses fell from these rocks, and now encumber the beach, rendering it difficult and dangerous to pass along shore. The concussion shook the earth, and the sound was heard in Bute and Argyleshire. On climbing towards the summit, found the whole of this enormous mass of strata, which reaches almost to the top of the mountain, had shifted from its original position; and that its transverse sections were separated from each other, so as frequently to leave spacious gaps between. In many cases too, the upper masses ride upon the ends of those below them; which, having no visible support, excite the most lively apprehension that the whole is about to fall, and crush the beholder to atoms. The Scriden rocks are reckoned the extreme point of Arran, towards north-east. Rocks fallen from the mountain.

Red sandstone, with sometimes pudding stone, continue along the coast, and ascend to the top of the mountain where they meet the schistus. Sandstone and puddingstone.

Near the Cock, these strata are intersected by a great vein of porphyry, the south-east side of which consists of rhomboidal, sharp-angled crystals of feldtspar, inserted in a red ground. On the opposite side, the ground is mostly intersected by porphyry.

blue basalt, with some streaks and spots of red intervening. This vein seems to rise to the top of the mountain; and it exhibits a curious specimen of different species of porphyry combined in the same mass. I do not see how the igneous theory can account for it; because, had it been raised from the bowels of the earth in fusion, it ought to have been homogeneous in its structure. The variety and separation of materials show they had been conveyed into a chasm in the strata, at different times, and from different sources; where they consolidated, by the attraction of cohesion, which operates upon bodies whose particles are brought into close union, by extreme pulverization. The earths, too, when minutely pulverized, combine with water, and pass from it, either in the form of cement to unite the grosser particles, or in the form of crystals. One point is clear, that the feldtspars in this porphyry had not, like those we have so often described, been conveyed to their present situation in a solid state, because their angles are not blunted, or worn. They must therefore have consolidated where they are now found.

Different species of porphyry in one mass.

The Cock is not a solid rock, as I expected, but a huge mass of sandstone that has fallen from the rocks, and stands on a narrow base upon the beach. It has acquired this name from mariners, to whom it serves as a land-mark, and to whom it presents a very lively representation of a cock crowing, and clapping his wings.

Cock, a huge mass of sandstone.

Contiguous to the Cock are irregular veins of basalt intersecting red sandstone, of a dark blue colour internally; but the sides next the sandstone are of a deep red colour, smooth and glossy like Roman bricks. The breadth of these veins varies from about one to three or four inches. Further on, the same sort of veins occur in red indurated clay shiver.

Red sandstone intersected by basaltes.

Several massy strata of indurated clay occur, of a florid red colour, to which the sea has conveyed a fine polish. From these, I am confident, vases, urns, jars, and all the more durable species of earthen ware, might be fabricated, susceptible of a jaspidean polish; and, being variegated with spots of different colours, might emulate the most elegant porphyry.

Red indurated clay, capable of a fine polish, and being wrought into ornamental vessels.

Within

Within tide are several strata that look like a Mosaic pavement of finely polished brick, of a florid red colour. They consist of equilateral pieces, or rhombi, of indurated clay, divided by septaria which run in straight lines parallel to each other, and are crossed by other straight and parallel lines with the greatest regularity, so that each rhombus is enclosed within septaria, which separate its sides from those of other rhombi, which are parallel to them. The breadth of the rhombi may be about fourteen inches; that of the septaria about half an inch. The colour of the septaria is a pale red, inclining to whiteness; and it looks very like puzzolatta cement, in the interstices between regularly formed stones. In many cases the pavement is worn down; but the cement, which is harder, preserves a regular equality of height. Natural Mosaic pavement.

I doubt much if the most skilful mason, or even a mathematician, could produce any thing more regular, or more beautiful.

Here are numerous strata of red clay shiver, and of red slaty schistus, which include various stratulæ of hæmatites, and of kidney ironstone. The hæmatites is generally arranged in stratulæ, but often interspersed. When stratified, it generally comes off in rounded pieces, whose sides are perpendicular, and are thicker towards the centre, than towards the circumference. When interspersed, the pieces are generally broader, are of a round, or oval form, and flattened towards the circumference. Iron ores.

The pieces that have been long exposed to the air are of a blood-red colour, though darker towards the centre. But by a little digging, pieces are found, with a red tegmen, but internally of a fibrous texture, the fibres at right angles to the breadth of the stone, and the colour that of steel. The interspersed pieces often exhibit an indented fracture, where the surface of indentations has the lustre of steel.

The kidney-form pieces often exhibit a dull red earthy appearance to their centre. But, where they have not been too much exposed to the atmosphere, they discover a radiated texture, with the lustre of steel.

These two species feel very heavy; but there are also

other stratulæ, which, though good iron stone, seem to contain a proportion of clay, and do not feel so heavy.

Used for marking sheep.

The people here call this ironstone *keel*, and use it for marking their sheep, and various other purposes. It has a greasy feel, and gives a stain to the hands, which it is difficult to wash off.

The farm of Cock is on a steep bank projected from the side of the mountain. The soil is mostly of a blood-red colour, and is composed of the debris of the schistus, and of the ironstone which it includes. It is wholly encumbered with this ironstone. Where burns had made excavations, I traced these ironstone strata to the summit of the mountain.

Imbedded in clay, under peat, on the summit of the mountain.

What is most extraordinary, great quantities of this ironstone are found, imbedded in clay, below peat bogs, on the flat summit of the mountain. This clearly shows that the summit was formerly covered by strata of red schistus, including ironstone, which have mouldered down. Much of the clay may have been washed away, leaving the ironstone, which water could not easily carry from a flat surface.

Blood-red limestone.

Connected with the latter, are several strata of limestone, of a florid brick, or blood-red colour. Such limestone always occurs, where hæmatitical strata present themselves along the coast. We shall therefore take no further notice of this limestone, until we reach Currie, beyond which it was not observed.

Jaspidean marble.

A stratum about three feet in thickness, and formed into blocks of from three to four feet in length, attracts attention. This stone contains innumerable shells, chiefly madrepores, some of which exhibit a bright crimson colour. The ground of the stone is somewhat calcareous, and in its fracture every way resembles jasper, of a chocolate colour. It may, not improperly, be called jaspidean marble. Were it polished, it would exhibit a striking appearance.

Change to sandstone.

Advancing onward, the strata upon the beach are suddenly changed. White and grey sandstone, with sometimes a mixture of yellow, occur, and are continued a great way.

In these the Cock coal is included. It is confined within an angular space, formed by two ledges, or edge-seams of limestone, one from north-east, the other from south-east, which meet at right-angles. To the north and south of these ledges of limestone, though the white sandstone extends a long way, and includes numerous beds of black bituminated shiver and blaes, no stratum of coal has been found. Coal.

There are three or four seams of coal running parallel from north to south; the principal, or main seam, being about fourteen feet in thickness. They dip nearly towards north-east, at an angle of 45°. Pits were sunk, and efforts used to work this coal, about fifty years ago; but as there is no harbour to export by sea, and a road must be cut several miles through rocks, a salt-pan was built, to consume the coal in the manufacture of salt. The undertaking seems not to have succeeded, and was soon abandoned. Attempts to work it fifty years ago.

The coal is of the same species with that at Kilkenny in Ireland; and there are similar strata in Ayrshire, in Fifeshire, and various parts of Scotland. It is blind coal, of uncommon excellence. It is not so apt to fall into powder as most other species, and, when fresh dug, it exhibits a metallic lustre. It is hence called *glance* coal by some; but this word does not distinguish it from some of its own varieties, which have no lustre; nor from some species of bituminated coal, which have a shining appearance. The word *blind* coal is more expressive of its peculiar property, in emitting neither flame nor smoke; as it consists of carbon, without any impregnation of bitumen. A blind coal like that of Kilkenny, of uncommon excellence.

I could not learn that the working of this coal was abandoned, because it 'soon disappeared,' as stated by Mr. Jamieson, p. 101; but that, from its inaccessible situation, they could not work it with profit. Boiling salt could hardly absorb their refuse, far less such a quantity as would keep the pit constantly going. It is well known, that if there be not a demand sufficient to absorb all that is turned out, no coal can be worked with profit; and that the working cannot be abandoned and resumed, according to the fluctuations of the demand; because, in that Why discontinued.

that case, men's wages would be running on, the machinery would be rotting, while the pit would be drowned with water.

Mr. Jamieson also states 'the great frequency of basaltic veins as another cause, which must render the coal, if it should ever be detected, of an indifferent quality, and difficult to work.'

No basaltes in the coal field.

With regard to the basaltic veins, I remarked it as an uncommon circumstance in Arran, that I could not find a single basaltic vein in the coal-field, or as far as the white sandstone extended on each side of it. There did not therefore appear to be the smallest ground for believing that the coal was cut off, or its quality injured, by basaltic veins.

Mistake of the miners.

When they wrought the main seam, by digging along its outcrop a large open trench, they came to the strata which form the steep side of the mountain, and which here rise at an angle of nearly 70°. They thought the coal extended through the base of the mountain, and cut a mine to follow it out. Had they bestowed the slightest attention, they might have seen, that the strata on the side of the mountain are very different from those which include the coal; and that, when the latter touch the former, they suddenly terminate. The strata on the side of the mountain appear to have been of much earlier formation, and they rise at a much higher angle, than those which include the coal. Nor does it appear that the quality of the coal was in the least affected by its approach to the mountain; for it was equally good until it was cut off.

Siliceous sandstone bent into Gothic arches.

On examining the strata perforated by their mine, I found them to be siliceous sandstone flags, of great hardness, and of a brownish white colour, their surfaces exhibiting micaceous scales. They are from one to two inches in thickness, and are curiously bent upwards, into the form of a Gothic arch, or rather of a great many arches overlapping each other, which form the roof of their mine. It was useless to follow the coal from the strata where it was found, into strata so very different, both in quality and position. As far as I could learn, the coal only failed them here, where they had no right to expect it.

Course of the coal, and man-

From the position of this coal, there can be no doubt but

but it expands to a great extent below the sea. If ever it be convenient to work it, I conceive it should not be by sinking pits, but by sinking in the stratum itself, and drawing it up the inclined plane of sandstone, on which it rests, by carriages running upon rollers. Manner in which it should be wrought.

This coal, being esteemed pure carbon, and capable of producing a most intense heat, I am surprised it never has been applied to the smelting of iron, and other fornacic uses. The ironstone here seems inexhaustible, and of excellent quality. A harbour might be scooped out of one of the ledges of limestone, which enclose the coal-field; which would cost nothing, as the limestone would repay the expense with profit. From this, the coal and ironstone might be exported to a situation favourable for an iron-work. With the ironstone might be made profitable.

Some strata of bituminated shiver, or shale, of a black colour, are visible, not only in the coalfield; but many are visible, for a great extent, on each side of it. These strata sometimes throw out an efflorescence of sulphate of magnesia. Bituminous shale.

But in one of the seams of coal which Mr. Cowie had wrought in the way called *open stretch*, for the purpose of burning limestone, I observed a highly bituminated species of till, of a black colour. This appeared so extraordinary, that I requested Mr. Cowie to write down a description of it, which follows. A stratum highly impregnated with bitumen between two seams of blind coal.

'The seam of coal, at the Cock of Arran, wrought by James Cowie, is first about ten inches, then eight or ten inches of a dauchy till, then twenty inches of coal. The dauch which separates the two seams of coal, is arranged in this manner like the back-bone of a fish, and rises in large pieces, but parts in the middle. The till between the two seams of the coal serves to burn lime.'

Mr. Cowie added, that the dauch was always mixed with the blind coal in burning lime. That it seemed to kindle more readily than the coal; and the only difference was, that the dauch always left a large *guest* (cinder); whereas the coal burnt into a fine white ash, of very small quantity. Mixed for burning lime.

Here, then, are highly bituminated strata of clay, not only connected with blind coal, but one interposed between two seams of that fossil.

Remarks on the vulcanean theory.

Our men of fire make their favourite element operate at many contradictions, as the hocus-pocus tricks imputed to phlogiston by the older chemists. At one time, phlogiston could not penetrate the most porous bodies; at another, the most dense were insufficient to confine it. At one time, it was the cause of gravity and attraction; at another, of levity and repulsion.

These gentlemen assert, that blind coal has had its bitumen evaporated, by the great heat which elevated the strata, from want of sufficient pressure to confine it; and that bituminated coal retained its bitumen, while subjected to this heat, in consequence of the enormous pressure which prevented its escape.

How came the bitumen here on this hypothesis?

But I would ask these gentlemen—How came the clay strata, in the same alternation with the coal, to retain their bitumen, while the coal was deprived of it? But especially, how came a stratum of clay, included between two stratum of coal, to retain its bitumen, while both the stratum of coal lost theirs? I do not see how these gentlemen can answer these questions, in a way consistent with their theory.

None in the sandstone over the blind coal.

They refer us to the sandstone which covers the blind coal, and allege we shall find some traces of the bitumen there. But though I examined the sandstone strata which formed the immediate roofs of the strata of coal, and many others, with the utmost care, I could not find the smallest visible trace of bitumen in them: nor could I trace the slightest mark of vegetable impression, either in the sandstone, or in the bituminated shiver connected with the blind coal.

Bituminous coal formed from vegetables,

Bitumen, particles of coal, and remains of vegetables, I have always found in the sandstone strata that covered bituminated coal; and often, in the coal itself, vegetable remains occur. Hence I inferred, that such coal had been formed from vegetables; and the marks I formerly assigned of sandstone, including coal, applied only to bituminated coal. Blind coal appears to be *sui generis*, and to have been formed without the aid of vegetables.

blind coal not.

But without pretending to assign the mode of its formation, I think I am warranted to assert, it was not formed

formed in the way our fiery philosophers allege; and that the facts stated are fatal to their theory, as far as it depends upon pressure, or defect of pressure.

The strata of white sandstone, and of bituminated shiver, occupy the coast only a short way, on the north of the coal field; but on the south they prevail for several miles.

IX.

Questions respecting the Vines and Wines of Champagne, by Mr. Chaptal, *with Answers to them by* Mr. Germon, *of Epernay**.

THE country that produces the celebrated wine known by the name of champagne is particularly famed for two kinds; the white, called wines of the river Marne; and red, or wines of the mountain of Rheims. If the southern aspects of the hills on the Marne produce excellent white wines, their backs and declivities, called the mountains of Rheims, though generally facing the north, and almost always the east, yield red wines of good and sound quality, and of a fine and high flavour, which ought to be made known. Red and white champagne wines.

The side toward Rheims is divided in trade according to the quality of its wines into the mountain, lower mountain, and St. Thierry. Of the first those of Verzy, Verzenay, Divisions into different qualities.

* The numerous facts here given render this Paper valuable, though the Author's theory and expressions are not always on a level with the present state of chemical knowledge. Nothing however more perfect, or more copious, has yet been published, respecting one of the three principal wine-countries in France. Mr. Chaptal will introduce almost the whole into his Art of Making Wine, which will appear in the course of the year.

[We have considerably abridged this Paper, from the Annales de Chimie, vol. LXI, p. 5, for January, 1807, taking only the principal facts; which we apprehend will not be unacceptable to many of our readers, as the practice of making wines for domestic use has much increased of late years; and many hints for the management of wines, and of the vine, may be derived from the information here given by a man of much practical knowledge.]

and

and Mailly, are most esteemed: the rest, though good, are not equal in quality. The vineyard of Bouzy, which terminates the chain, and the horizon between the south and east, so that it belongs to both divisions, must not be omitted. It produces excellent red wines, participating, from its situation, in the good qualities of those of Verzenay, and the good red wines of the Marne.

The lower mountain comprises a great number of vineyards, among which are distinguished those of Chamery, Ecueil, and Villedemange. The latter in particular produces wine in a favourable season, that will keep ten or twelve years. This division extends to the banks of the Aisne, but it produces only common wines.

The wines of St. Thierry are very pleasant, of a light colour, and much sought after in commerce. But the *clos* St. Thierry, from the archbishopric of Rheims, is the only one that unites the colour and flavour of burgundy with the lightness and briskness of champagne. It is to the champagne wines what the Clos-Vougeot is to burgundy.

What is the best aspect for vineyards?

The best aspect for vineyards is unquestionably the east and south. Of situations the midway of a hill is preferred, as the heat there is more concentrated, while it is exempt from the variations of the air on the summit, and the damp vapours of the foot. A western aspect is unfavourable to vegetation, which it burns and dries up: so that a vineyard with an eastern aspect is more valuable by one third.

What is the best soil?

Next to aspect in importance, if not before it, is the nature of the soil. This should be light, sandy, and granitic; neither compact, close, nor clayey. In general the vineyards of Champagne have a substratum of chalk: a kind of soil on which the vine grows slowly, but when it is once thoroughly rooted on it, it thrives well.

When and how are the vines planted?

The vines are planted in November or December, when the weather permits. An oblong hole or trench is made a foot and half deep, and two or three feet long. Into this the plant is introduced, and covered with earth, inclining it so, that only two or three inches of the extremity rises above the surface; and this extremity is *refreshed* by cutting it lightly in a horizontal direction. These trenches are made in rows, a foot and half from each other in strong grounds,

grounds, and two feet in light. A distance of three feet is left between the rows, and the plants in one are placed opposite the intervals of the other.

They are propagated by layers: for which purpose a turf should be cut from a meadow, or a marsh; the branch to be laid should be introduced into a hole made in the middle of this turf, and then fixed in the ground with it, in a sloping position. The root will form in the course of the year, and then the layer must be cut off close to the stock, and taken up with its turf. Propagation by layers.

Grafting is almost out of use. The fruit indeed is larger, but it is much more liable to fail from the slightest mismanagement, does not produce so sound a wine, and the vines do not last so long by far. Grafting disused.

A good vineyard will continue to produce well for fifty or sixty years, and frequently more, if well managed. If the layers be not planted deep enough, the vineyard will be covered with trailing roots, forming a sort of floor, so that there will be no place to lay down fresh shoots, and it must be broken up. How long does a vineyard last?

For white wine black and white grapes are planted indiscriminately in the same vineyard, which is perhaps wrong, as they do not ripen at the same time. But wine made entirely from black grapes would be too strong, and apt to become pricked in hot years: and entirely from white it would be too mellow, as these grapes contain more mucilage than the black. What grapes are most proper for white wine?

The kinds of grape cultivated are not many. The black are generally preferred for several reasons. They are not so soon spoiled by frost or rain, which are common about vintage-time; and they give more strength and body to the wine. Yet there are some places, where the wine is much esteemed, though they have few black grapes. Black generally preferred.

As there is no danger from the frost in spring but at sun-rise, an eastern exposure has most to apprehend from it, but no aspect is exempt from the danger. No means of guarding against it have yet been discovered. Danger from spring frosts.

On resuming the labours of the vineyard, about the end of February or in March, the first thing to be done, and one of the most essential, since on it depends the greatness Management of the vineyard.

of

Pruning. of the crop, is to prune the vine. When it is strong, two side shoots or branches may be left: if weak, only one. Three eyes should be left on each shoot. Sometimes the vinedressers leave three shoots, and four eyes to a shoot. If the vine be young, and the stock is not loaded with old cuttings, its height when pruned is only three or four inches. The branches are never suffered to shoot up above a foot and half.

Digging. After this pruning, about the end of March, or in April, when the earth is softened and rendered pliable by the winter frost, it is dug up about a foot deep, so far as to uncover the roots, and all the clods are well broken.

Putting down layers. After this layers are put down where necessary, throwing on them a basket of dung, and filling up the trench with mould; so as to let the two or three stems left on them appear above ground four or six inches apart, and taking care not to injure the buds on them.

Stirring the ground. At the end of April, or in May, the earth is stirred again, but more superficially.

Stopping the shoots, and staking. While the vine is in flower, it must not be touched: but when this is over, in June, the shoots are to be stopped at about a foot and half; and the vine is to be staked, and tied up, but not so as to interrupt the circulation of the air, or the developement of the shoots.

Stirring the ground again. When this is done the earth has a second stirring, and about the middle of August, it has its third and last.

Ripeness of the grapes. About the end of September, or later in some seasons, the grapes will be ripe; which is known by the footstalk of each being brown and woody, as well as the general stalk; the grape coming off easily, and the part of the stalk within it not appearing green; and the stones being brown, dry, and not glutinous.

Gathering them for white wine. Great care is necessary for making the white wine. The ripest and soundest bunches must be carefully gathered, freed from all dry, rotten, and bruised grapes, put into large baskets, covered with a cloth to keep the sun from them, carried into the shade, and there kept till the evening, when they are to be pressed as speedily as possible.

Pressing. The grapes being laid on the bed of the press, they are to be covered with three or four layers of flat stones, and the press turned. When the juice has run for four or five minutes,

nutes, the press is to be turned backward, the stones removed, the grapes that have protruded thrust into the heap, the stones replaced, and the press turned again. The juice from three of such pressures, which will not take up an hour, is put by itself for prime wine into a vat, where it is left all night to settle.

Putting into casks.

The next morning this juice is poured off from the sediment, and put into new, matched, and well rinced casks. In these it ferments, at first violently, afterward imperceptibly; till, about the end of December, having gone through all the stages of depuration, it becomes fine. It is then racked off, in dry weather, and on some fine frosty day, and fined with isinglass. About a pound of that of Marseilles is sufficient for 40 puncheons, of 200 bottles each. The isinglass being dissolved is well beaten, diluted with wine taken from the cask, then poured into it, and the whole well stirred by an instrument introduced at the bung-hole. The wine thus left to settle ferments slightly again, till it is stopped by the cold weather, or by time. In a month or six weeks it is racked off again, and has another fining with half the quantity of isinglass.

Racking and fining.

Second racking and fining.

Bottling.

In this state it usually remains till March, when it is bottled. Good glass bottles are taken for this purpose, well rinced, corked with superfine corks, and these are confined by packthread or wire. The bottles are then piled on their sides, one upon another, in the cellar.

As the fermentation is not completely terminated at the time of bottling, it revives about the middle of August, between which time and the end of September it is not unusual to have five or ten bottles in a hundred burst; and this continues till the March following, when it becomes more violent, or more moderate, according to the state and quality of the wine. In general, when not more than twenty bottles in a hundred burst during the whole course of the fermentation, the proprietor does not complain.

Decanted into fresh bottles.

Fifteen or eighteen months after the first bottling, when the wine has gone through all the stages of fermentation, and is to be sold or sent abroad, it undergoes a fresh decantation, which requires some dexterity. If the wine be not mantling [*mousseux*], it is simple enough. The bottle

is

is taken up in the position in which it lies; the wire is removed with a hook, which the man holds in his hand; the cork is drawn; and a well rinced, empty bottle being held perpendicularly to its mouth, the wine is decanted, leaving at the bottom the sediment, which had not been shaken. Some employ a siphon to draw off the wine in this case.

When the wine is mantling, the operation is much more nice and tedious. Boards are prepared, with holes at certain distances to receive the bottles, and placed near the pile. A workman carefully takes a bottle from the pile, in the position in which it lay; shakes it with a gentle, slow, and regular motion, so as to get all the sediment down to the side of the bottle, and thence to the neck, without mixing with the rest of the wine; and then places it on the board in a sloping position. This is done regularly till the board is filled. Twenty-four hours after the bottles are moved again in a less inclined position, so as to bring the sediment down upon the cork. If this be done completely, without rendering the wine at all thick, it is placed in a perpendicular position, and the same is done with all the other bottles. He then takes them one by one, bottom upwards, stays them with his left arm, removes the wire with his hook, and carefully draws the cork. The fixed air expands; the wine forces out the sediment into a receiver; when instantly the workman turns up the bottle, which has let out only what was necessary to render the remainder perfectly clear, and gives it to another, who fills it, and recorks it.

Will keep from ten to thirty years.

This wine, when sent abroad, will keep ten years, without its quality being impaired: but in cellars, particularly those of Champagne, which are cut out of the chalk rock, it will keep twenty or thirty years. The temperature of the cellars should be equable, and currents of air in them avoided.

Gathering the grapes for red wine.

For making red wine, the grapes are gathered with the same precautions as for making white, taking only the black grapes. These are bruised in particular vessels, by

Treading them.

men treading on them with strong wooden shoes: part of the stalks are thrown away, and the must is left in covered

Fermenting.

vessels to ferment sufficiently to extract the colouring matter from

from the pellicles. In some years three or four days are sufficient for this; in others it requires ten, fifteen, or even twenty.

When the fermentation begins, the husks and stalks are forced down so as to be entirely covered with the must, either by means of stout poles furnished with cross pegs; or, which is better, by a couple of stout men going into the vat, and well treading and mixing its contents. When the air above the vat extinguishes a candle; the stalks and husks rise forcibly, whatever pains be taken to sink them frequently, that the must may not acquire a disagreeable taste; the must experiences a degree of real ebullition; and the colouring matter is sufficiently decomposed: the fermentation must not be carried farther, lest the wine acquire a dry and hard taste, not to be cured even by keeping. Management of the fermentation.

The liquor is then to be drawn off into another vat; and the marc pressed, but only twice or three times. What runs from the marc being well mixed with the other, the whole is to be tunned into new, well hooped casks, previously rinced with hot water; but these must not be quite filled, as the wine still ferments for some days. As the fermentation abates, they are filled up, and the bung, in which a little hole is made, is put in. When the fermentation is become imperceptible, the cask is stopped close; care is taken to fill it up from time to time, for there is soon a vacuity formed in it, and even to open the bung. Pressing. Putting into casks.

About the end of December, in dry weather, and if possible on a fine frosty day, because all fermentation has ceased, the wine is racked off from the lees. About the middle of May, before the hot weather comes on, it is racked off again, and the barrels are fresh hooped, and the wine is put into the cellar. Racking. Second racking.

When the wine is to be sent off to the consumer, or put into bottles, it is fined. For this purpose the wine is racked off a third time; and the whites of five or six very fresh eggs are well beaten up in a pint of water, without making them froth if possible, for every puncheon holding 240 bottles. Those for white wine hold only 200. This is put into the cask, and stirred about, as in fining white wine. Third racking and fining.

The wine is generally bottled in November, or thirteen months Bottling.

months after it was made. Some excellent and generous wines may stand on the lees three or four years; but they should be kept in barrels that will hold eight or ten hogsheads, when the wine will feed, and be the better for it.

Keeps from six to twelve years.

Good red champagne will keep in bottle six, eight, ten, or twelve years.

Management of tall vines.

In some places, the vines are suffered to grow much higher than in others, or to about five feet, but this is adapted only to a strong and vigorous soil. For this purpose the strongest shoot of the vine is taken, all the rest being cut off, and all the lateral shoots. This is bent round in a complete circle quite to the stock, at the time when the sap is most abundant, and the buds already opened; and supported by an oaken prop six feet high, and an inch square, to which it is fastened in two or three places.

Laying down.

Vines of this kind are propagated by laying down the old stocks every ten or fifteen years, in a small long trench; leaving on them three or four branches, which are likewise buried in the ground. These will produce good plants for the following year.

Produce greater but not so good.

The produce of these vines is greater, but the grapes do not ripen so early, and the wine in consequence is not quite so fine and exquisite.

X.

On the most sensible Reagents for Muriatic, Carbonic, and Sulphuric Acids, and for Ammonia: by C. H. PFAFF, *Professor of Chemistry at Kiel* *.

Sensible test of muriatic acid desirable.

IN the inquiry concerning the pretended formation of muriatic acid in water, by means of the galvanic pile, it is unquestionably of great importance, to possess a very sensible test of this acid, that we may discover the first traces of it, and pursue its successive increase. Hitherto the nitrate of silver has generally been employed. This reagent is no doubt very sensible to this acid. Kirwan asserts, that one part of the acid diluted with 108333 of water may be de-

Nitrate of silver.

* Annales de Chimie, vol. LXII, p. 19, April, 1807.

tected

tected by its means; but this test is greatly surpassed by the solution of *mild nitrate of mercury* prepared cold. One part of muriatic acid of the specific gravity of 1·15, diluted with 70000 parts of water, barely exhibits a slight opaline hue, when tested with nitrate of silver. Diluted with 80000 times its weight of water, it eludes the action of this test, as well as of all others, except the mild nitrate of mercury, which renders it very perceptibly turbid. Its sensibility is so great indeed, that even $\frac{1}{100000}$ of a grain of muriatic acid at 1·15 is indicated by a slightly dull tint in the water that contains this extremely small quantity. From reflecting on the absolute insolubility, as it may be called, of the mild muriate of mercury, I was led to experiments concerning this reagent.

Mild nitrate of mercury much superior.

Indicates $\frac{1}{100000}$

It is at the same time the most sensible test of ammonia. One part of this alkali, diluted with 30000 of water, is indicated by a slight blackish yellow tint, when a solution of nitrate of mercury at a minimum of oxidation is added to it.

Detects $\frac{1}{100000}$ of ammonia.

Lime water, or barytes water, is generally considered as the most sensible test of carbonic acid. I have found, that the acetate of lead surpasses both. I was led accidentally to make this observation. Some distilled water, which I kept in a cellar not very deep under ground, where however there were no fermented liquors, was rendered very sensibly turbid, by adding this solution. Kirwan has accused the acetite of lead of being a deceitful test, his solution, which had been kept a little time, being sometimes rendered turbid by pure distilled water. But it is not deceitful; the water in this case is not pure, it contains a little carbonic acid. I prepared some distilled water free from all carbonic acid. It was not rendered turbid either by limewater, or by the solution of acetite of lead. I passed into it a few bubbles of carbonic acid, which acidulated the water so slightly, that it neither reddened litmus paper, nor rendered limewater turbid; but the solution of acetite of lead whitened it perceptibly.

Acetate of lead most sensible test of carbonic acid.

Not deceitful.

Acetate of lead is much less sensible to other acids. A solution of sulphuric acid at 1·85, diluted with 16000 parts of water, which acts sensibly on litmus paper, is not rendered turbid by acetate of lead. Barytes water however

Not so sensible to other acids.

detects $\frac{1}{70000}$ of its weight in water acidulated with sulphuric acid, and surpasses in sensibility for this acid all other reagents.

Mild nitrate of mercury a test of phosphoric acid.

The mild nitrate of mercury is almost as sensible a test of the phosphoric as of the muriatic acid; with this difference, that the precipitate with the former is soluble in an excess of phosphoric or nitric acid, but that with the latter is absolutely insoluble in an excess of any acid whatever.

XI.

Some farther Remarks on the pretended Formation of Muriatic Acid in Water by the Influence of the Galvanic Pile: by Professor PFAFF, *of Kiel.*

Inquiry into the formation of muriatic acid by galvanism. Apparatus.

I HAVE continued my researches into the pretended formation of muriatic acid in water, by the influence of the positive pole of Volta's pile. I have employed glass tubes of various diameters, from one line to an inch. The tubes were closed at bottom, into which the conducting wires were cemented with sealing wax. The communication between the two tubes, into one of which the influence of the positive pile was conducted, while that of the negative communicated with the other, was made at the top, sometimes by wet paper, sometimes by linen threads, sometimes by tendons, and sometimes by muscular fibre. I likewise varied the metal of the wires, employing successively platina, gold, silver, copper, and iron.

No traces of muriatic acid.

In all my experiments I could never obtain the least trace of muriatic acid, though my test, the mild nitrate of mercury, the most sensible of all for this acid, would have indicated the presence of $\frac{1}{100000}$ of a grain. But I found by litmus paper indications of an acid; which certainly was neither the muriatic, sulphuric, carbonic, nor phosphoric; since the nicest tests of these acids, which greatly exceed litmus in sensibility, gave no signs of their presence. In all probability therefore, it could be nothing but the nitric acid.

Some acid however.

Probably nitric.

Ammonia too. Both from the azote in the water.

I always obtained traces of an alkali too, which from every test was ammonia. I cannot therefore but adhere to my opinion, that the acid and alkali are formed at the expense of the nitrogen adhering to the water; which on one side unites with oxigen, on the other with hidrogen.

XII.

XII.

Description of the Mode of making Threshing-Floors in the Commune of Valbonnais, in the Department of the Isère: by Mr. J. J. CHAMPOLLION FIGEAC, *Secretary to the Society of Sciences and Arts at Grenoble, &c.* *.

THE gypsum quarries of Valbonnais furnish two sorts of plaster, one white, the other red. The white is found only in solitary strata, not very abundant: the red, which is coloured by oxide of iron, is the most plentiful, and used almost exclusively for threshing floors.

Two sorts of gypsum. The red used for floors.

For this purpose it is burned for 24 or 30 hours, pounded as fine as possible, and in this state left for ten days, before it is used. It is to be observed, the more it is burned, and the finer it is pounded, the better it is. At the expiration of this term, and after the ground on which the floor is to be formed has been made very level, the plaster is to be diluted with cold water in a bucket. It must be carefully mixed so as not to have any lumps.

Calcined, powdered, exposed to the air, well mixed with cold water.

Two feet from one of the walls of the barn, and parallel to it, a ruler is to be placed, of the height which the plaster floor is intended to have. This is commonly two inches and half, or three inches. When the plaster is quite smooth, has acquired a certain degree of consistency, and is almost beginning to dry, it is poured out on the space between the wall and the rules. To level it another ruler is passed over it, one end of which rests on the former, the other touches the wall. It is then gone over with a trowel, to make it as smooth as possible, every vacuity is filled up, and any heterogeneous matters, that may be on the surface, are removed. Thus a smooth level surface is given to it, which is an essential quality.

A slip of wood placed two feet from the wall, and the plaster just hardening laid in the interval and smoothed.

As soon as this is done, a similar quantity of plaster prepared in the same manner is laid at the end of the former, and the same operations are repeated, till the plaster is extended to the opposite side of the barn. Here, however, it is absolutely necessary, to leave a little void space,

This is repeated till within a few inches of the opposite wall.

* Sonnini's Bibliothèque Physico-économique, Feb. 1807, p. 315.

to guard against the inconveniences that would ensue from the plaster swelling when in contact with both walls. This space may be three inches in a length of twenty feet. Other layers of plaster are then formed in succession by the side of this, bounding them always by the long ruler, placed at two feet distance from each preceding layer, which will keep them all of an equal thickness; and thus the whole of the floor is completed.

The board removed two feet farther, and another layer formed; and this repeated till the space is covered.

Great care must be taken, that the successive portions unite well together, that there may be no vacuity between them. For this purpose it is necessary to finish the whole in one day if possible; and to accomplish this a sufficient number of men should be employed in diluting and preparing the plaster, that those who are forming the floor may proceed without interruption.

Every portion must be well united, and the whole finished in a day if possible.

Ten days afterward the vacuity left between the floor and the wall is filled up, and then it will be ready for use. If in this time it acquire a deep red colour, it is a good sign. Such a floor will last in common a hundred and fifty years; and still longer, if it be not exposed to damp. When its surface becomes injured by time, and is no longer as smooth as it ought to be, all the plaster may be removed, exposed to the weather for a fortnight, burned again as if it were fresh taken from the quarry, pounded, mixed with water, and relaid in the same place, proceeding exactly in the same manner as when it was laid down the first time. The floor thus remade will last as long as it did before.

Ten days after the vacuity filled up.

Will last 150 years,

and then may be taken up, burned afresh, and laid down again.

Will then last as long as before.

The advantages of such a floor may readily be conceived, when the high price to which timber has risen of late years is considered. That some calculation of its cost may be formed, a square fathom of this floor, three inches thick, will require about eleven hundred weight of gypsum; and two men can work up seven times this quantity in a day.

Quantity of materials and labour.

SCIENTIFIC NEWS.

French National Institute *.

MR. LAPLACE has investigated the phenomena of capillary attraction; but instead of copying what Mr. Delambre says on this subject, we shall refer our readers to p. 164, 169, and 286 of the present volume for what has been done by this celebrated mathematician; and to our next number for some remarks on it by a learned correspondent. Laplace on capillary attraction.

In 1784 Mr. Roswag of Strasbourg presented to the board of trade some gauze made of iron wire, for which he received a reward; and the loom he invented for making it was lodged in the collection of machines of Vaucanson. In 1799 Mr. Rochon made others, and coated them with a transparent glue, to be substituted instead of horn for ship lanterns to be used between decks, and in engagements by night. He has since conceived, that with a thin coating of plaster they might be employed to preserve ships from fire, and buildings on shore still more easily; or at least that they would render the ravages of fire less frequent, and less terrible. These gauzes might be very useful too for theatrical decorations, which would not be liable to take fire. Their only inconvenience is their being so little flexible; but Mr. Rochon does not despair of means being found by chemistry to remedy this imperfection, and it was with a view of calling attention to this subject, that he read a paper on it to the class. Wire gauze coated with glue as a substitute for horn in ship lanterns. Security against fire. Safe stage decorations.

An eclipse of the sun is among the most useful phenomena for the verification of astronomical tables, or for the determination of the longitudes of places. It is likewise one of those, that most attract the attention of observers. Mr. Lalande, true to the custom he has followed these fifty years, has calculated all the observations he could collect of the eclipse of 1806. Clouds concealed it from the astrono- Total eclipse of the sun.

* Abridged from the account of the proceedings of the mathematical division of the class of mathematical and physical sciences given by the perpetual secretary, Mr. Delambre.

Observed in America.

mers of Paris; but it was seen in several parts of France, Germany, Holland, and Italy. In America it would be particularly interesting, as at Boston and Albany it was total. At Kinderhook, near Albany, it was observed by Mr. Ferrer with excellent instruments. He concluded the conjunction to be at 45 min. 33 sec. after 11. Mr. Lalande found precisely the same: and as he learned by other observations, that it happened at 30 min. 6 sec. after 4 at Paris, it follows, that the difference of longitude of these two places must be 7h. 15′ 27″ of time.

The eclipse was observed at Albany too, but at the instant of the return of the light the observer had not his eye at the glass: and though this phenomenon would appear to be of a nature to be seen as accurately with the naked eye, it seems to have been noticed a few seconds too late.

Disk of the moon illumined from its atmosphere.

A curious remark of Mr. Ferrer is, that the disk of the moon appeared illumined a few seconds before the end of the total eclipse, which seemed to him an effect of the atmosphere of the moon.

Only six stars visible, the sun being surrounded by a luminous ring.

The darkness was not so great as was expected. Only six of the principal stars or planets were seen. A luminous ring of 45 or 50′, surrounding the sun, diminished the obscurity.

Irradiation of the sun 2′. The diameter of the moon 2″ more than Lalande had calculated.

From the comparison of this total eclipse with some annular eclipses observed before, Mr. Lalande thinks, that the irradiation of the sun is 2″; and that 1″ must be added to the semidiameter which he had assigned to the moon from direct observations made at the full.

The sun movable in space,

Many astronomers have supposed, that the sun is not immovable in space. Mr. Lalande conjectured from its rotatory motion, which is unquestionable, that it has a movement of revolution. What he suspected Herschel has endeavoured to prove by observations. Mr. Prévot, of the academy of Petersburg, has been led to the same result: but Mr. du Séjour, having treated the question analytically, has found, that it is insolvable when considered in its totality. The results to which Mr. Herschel has been led by the apparent motions of different stars* do not accord suffi-

*See our Journal, vol. XIII, p. 59, and XV, p. 232, and 269.

ciently

ciently to establish the motion of the sun, and immobility of the stars: it rather appears, that they are all in motion: and it is on this supposition, that Mr. du Séjour declares the problem insolvable.

and the stars likewise: so that its motion cannot be determined.

Notwithstanding this decision, Mr. Buckhardt has subjected it to analysis anew. His formulæ are more commodious, and more easy of application, than that of Mr. du Séjour; and are less laborious than the trigonometrical calculation of Mr. Herschel. He has very adroitly eliminated the distances of the stars, which appear to be, and really are, one of the elements of the calculation, and which will probably remain for ever unknown to us. If the sun alone be in motion, this motion may be known to a certain degree of accuracy, in time, by means of good observations: but if the stars too move, the separation of the unknown quantities will be impossible, and some embarrassment will ensue to future astronomers, should there be an interruption to observations for a few centuries; and should they attempt to calculate the celestial movements anew by comparing their observations with ours, after a period of ignorance of some duration. But even on this supposition, which is fortunately very improbable, it would only follow, that the observations of the 18th century would appear a little less accurate; which would not prevent them from furnishing much better helps, than we found in the small number of rude observations transmitted to us by the Greeks.

Burckhardt has examined the question anew.

The problem of finding the train of wheels necessary to represent the motions of the planets was resolved by Huyghens in a very complete manner by continual fractions, which have the advantage of furnishing approximate values, expressed by the smallest numbers possible, in every degree of approximation with which the artist may think proper to content himself. But to this every artist who attempts to construct a planetarium is not equal. Mr. Burckhardt therefore has pointed out to them calculations more easy, and sufficiently exact.

Problem of finding the train of wheels for a planetarium.

To these labours of the class may be added the reports made by its committees on the most curious and important inventions submitted to its judgment. On both these accounts we shall particularly mention the report of Carnot

Pyreolophorus.

on

on the machine invented by Messrs. Nieps, and called by them a *pyreolophorus*. By this word, compounded from πυρ, fire, αιολος, wind, and φερω, to carry, the inventors intended to point out the moving powers of the machine, which are wind from a pair of bellows, and air suddenly expanded by fire *. Their object was to discover a physical power equal to that of the steam engine without consuming so much fuel.

A power equal to the steam engine with less consumption of fuel.

To form an idea of the manner in which they produce and call into action the sudden expansion of air, suppose a copper receiver to be firmly fixed to a horizontal table. To one of its sides is fitted a tube, by means of which a body of air is conveyed into the receiver. This air meets in its way a few grains of combustible matter, which it projects on a flame, where it enters into ignition. The inflamed matter, entering into the receiver, expands its contained air with great force, which is exerted against the sides, and pushes forward a piston, sliding in a second tube, fitted to one of the sides. This piston drives before it a column of water, or any other body exposed to its action; after which the piston returns of itself to its former place, and the machine, recovering its former state, is again ready to act as before. All these effects take place in five seconds of time.

Its mode of action.

In a trial made by the inventors, a barge loaded with nine hundred weight, and its bow presenting a resistance of six square feet to the water, ascended the Saône with a velocity double that of the stream. In another trial made by the committee, the pressure exerted on a piston of 3 inches square was equal to a weight of 57000 grammes (126lbs); the interior capacity was 21 cubic inches; and the consumption of fuel was only 0·32 of a gramme (5 grains).

Experiments with it.

The inventors mean to carry their first attempts nearer to perfection: but even in the present state of the machine, its violent concussions, the shocks it gives to what supports it, and the celerity of its motions, leave no doubt of the in-

* This principle has already been employed in our own country, and we understand its powers were found to be very great; but some obstacles occurred, that prevented it from being followed up. From the account given by Messrs. Nieps, however, it was not precisely in the same way as their contrivance, but on a simple and more scientific principle. W. N.

tensity

tensity and impetuosity of this new moving principle; and valuable results may be expected from it, when by repeated trials all the energy of which it is susceptible is imparted to it. Such is the opinion of the committee, and the class determined, that the whole of their report should be inserted in the historical part of its memoirs, to preserve the remembrance and date of the first trial of an invention that may prove highly important.

New scapements.

Mr. Pictet presented ten models of scapements from Messrs. Malley of Geneva, three of which belonged in part to Mr. Tavan, the artist who made them all. They displayed an inventive genius, and great merit in the execution.

Frame for weaving ribbed stockings.

Mr. Desmarets read an interesting report on a new machine for weaving ribbed stockings, invented by Mr. Bellemère. This is not above half as expensive as the English stocking-frame, and its movements are much lighter. Its advantages are confirmed by two years experience.

Variation and dip of the needle, and magnetic power.

From the learned researches of Mr. Coulomb, and the formulæ of de Borda and Laplace, we are now able to determine with sufficient precision, and without too many difficulties, the variation and dip of the needle, and the intensity of the magnetic forces. But these nice observations require perfect instruments, time, and an exact knowledge of the meridian of the place. The observations which navigators, to whom most of these are often wanting, have been able to make, are not to be depended upon sufficiently for us to infer from them with certainty the situation of the magnetic poles and equator, and the points where the magnetic equator intersects that of the earth. Mr. Biot however has attempted to determine, from the observations of la Peyrouse and von Humboldt, all these elements of the magnetic theory of the globe; and he has given the necessary formulæ for calculating what the variation and dip of the needle should be in any given place.

Mr Biot has attempted to ascertain the elements of the magnetism of the globe.

Humboldt and Gay Lussac have made many observations on the dip of the needle, and the magnetic power of the earth.

The journey which Messrs. von Humboldt and Gay-Lussac have since made in Italy, France, and Germany, has afforded them repeated opportunities of comparing their observations with the hypothesis of Mr. Biot. The difficulty of ascertaining the meridian prevented them from observing the variation of the needle at their different stations; but

but they observed the dip, and the number of oscillations made in a given time by a horizontal needle, whence by a very simple formula they deduced the number of oscillations it would have made in its true direction, and the intensity of the magnetic forces.

These did not coincide with Biot's hypothesis.

To exhibit the whole of their labour at one view, and the consequences deducible from it, Mr. Gay-Lussac has given a general table of the observations themselves, the geographical latitude and longitude of the place, the latitudes and longitudes referred to the magnetic equator according to the hypothesis of Mr. Biot, the dips calculated according to this hypothesis, and the differences they found between their observations and this calculation. To this he has added observations on the nature of the soil, and its elevation above the level of the sea.

The dips were all in excess.

It is to be remarked, that all these differences are in the same direction, the dips by calculation being from 3° 42′ to 5° 9′ too great. Admitting, that some of these differences may be ascribed to local circumstances, or the unavoidable errours of observation, it appears at least highly probable, that a more considerable part arises from the situation attributed to the nodes of the magnetic equator, and to the angle it makes with that of the earth. It will not be difficult to determine the corrections, that Mr. Biot's hypothesis requires, to agree much better with these new observations, and reconcile them with those from which he determined his first elements. It is to be presumed, that Mr. Biot himself will consider this as an object of sufficient importance to engage his attention, when he has finished the important and difficult undertaking, on which he is now employed *. To give this theory all the precision of which it is susceptible, it is much to be wished, that we had a series of observations made in remoter parts of the globe with the same care as those of von Humboldt and Gay-Lussac: but in the mean time we perceive, that the intensity of the

Meridian line extended to the Balearic islands.

* Messrs. Biot and Arago set off in September last to continue the meridian line to the Balearic islands, and finish the labours interrupted by the death of Mr. Mechain. In December they began the observation of the great triangle, which is to connect the island of Ivica with the coast of Valencia.

magnetic

magnetic forces increases with the latitude, as Mr. von Humboldt had already remarked on his American tour; for at Berlin it is 13703, while at Rome it is only 12642. It follows too from their labours, that the influence of the chain of the Alps was very feeble, if any thing. That of Vesuvius at the moment of the earthquake and eruption of 1805 was not much more perceptible, and this would appear to be owing rather to local circumstances, than to a particular magnetic centre.

The magnetic power increases with the latitude.

The influence of the Alps little or nothing.

That of Vesuvius during an eruption the same.

The description of the instruments employed in these observations, and the disquisitions entered into by Mr. Gay-Lussac respecting the best means of making them, cannot fail to add to the confidence, which the well-known accuracy and skill of the observers must naturally inspire.

From eudiometrical experiments, and the analysis of the air, Messrs. von Humboldt and Gay-Lussac had been led to suspect, that all gases might have the same capacity for caloric. This consequence, which appeared deducible from their observations, deserved a more scrupulous examination, which Mr. Gay-Lussac undertook on his return. His new experiments confirmed those before made, yet led him to an opposite conclusion. The gases he had observed with Mr. von Humboldt had in reality nearly equal capacities for heat, but it was wrong to ascribe the same property to all gases without distinction.

All gases supposed by Humboldt to have the same capacity for heat;

but this is true only with respect to some.

The apparatus contrived by Mr. Gay-Lussac was extremely simple. It consists of two equal globes, each with two tubulures, one fitted with a cock, the other with a very sensible spirit thermometer. The globes having been freed from moisture by dried muriate of lime, they were exhausted of air, and one was filled with the gas to be tried. The communication between the two balloons being then opened, part of the gas included in the first rushed into the second, till an equilibrium was established; and then Mr. Gay-Lussac carefully examined the changes of temperature indicated by the two thermometers.

Gay-Lussac's apparatus for determining this.

In the first experiment, the subject of which was atmospheric air, he saw with astonishment the thermometer rising perceptibly in the exhausted globe in proportion as the air rushed into it. This fact appears diametrically opposite to another

Air rushing into a vacuum gave out heat,

another well known, which is, that a volume of air included in the receiver of an air-pump continually absorbs caloric as it dilates under the rising piston. It may be said, that the vacuum in the second globe was not sufficiently perfect, and that the air remaining in it, being compressed by the additional quantity admitted, was obliged to give out part of the air it contained: but this explanation Mr. Gay-Lussac refutes, first by reasoning, and afterward by a direct experiment.

and this in proportion to its densit/.

If the alcohol ascend in the second thermometer, it descends nearly the same quantity in the first. Now if, after having exhausted the second globe, the communication between them be opened, the gas, equally distributed, will be reduced to half the density it had before; and one of the thermometers will be seen to rise, and the other fall, each in an equal degree, but less than before, in consequence of the diminution of density. And if, by repeating the exhaustion, the density be reduced to half what it was in the second trial, and consequently to $\frac{1}{8}$ what it was in its original state, we shall find the equal and opposite variations of the two thermometers still following the ratio of the density.

Other gases produced the same phenomena.

Similar experiments, made with particular attention, on hidrogen, oxigen, and carbonic acid gas, produced similar results; that is to say, the quantities of caloric absorbed in the first globe, and evolved in the second, were always equal to each other, and proportional to the density.

Contrivance for equalizing the time of transmission of the gas.

To render the experiments comparable with each other, it was necessary, that the time occupied by all the different gases in their transmission from one globe to the other should be the same. This Mr. Gay-Lussac effected by a contrivance equally simple and ingenious, which diminished the orifice of the connecting tube in the ratio of the square root of the densities: and thus the time of transmission for all the gases was found to be eleven seconds.

Of these experiments, which deserve the attention of the natural philosopher, and which Mr. Gay-Lussac purposes to verify and extend by farther observations, the following are the results, which however he offers with some diffidence.

General results of the experiments.

1. When a vacuum comes to be occupied by a gas, the caloric evolved is not owing to the little air that might be left in it.

2. If

2. If a communication be opened between two equal spaces, one a vacuum, the other filled with a gas, the variations of temperature, positive in one and negative in the other, are equal in quantity, but not in intensity.

3. In the same gas these variations are proportional to the change of density it undergoes.

4. The variations in different gases are so much greater, in proportion as their specific gravities are less.

5. The capacity of a gas for caloric in a given volume diminishes with the density.

6. The capacities of gases for caloric, in equal volumes, are so much greater, as their specific gravities are less. This consequence will be evident to those who know the experiments, by which Mr. Gay-Lussac had already proved, that all gases are equally affected by equal elevations of temperature.

Progress of thermometers compared, both mercurial and spirit,

Mr. Cotte, correspondent of the Institute, has compared the progress of several thermometers, both of mercury and alcohol, in various expositions, during the hottest days of the three memorable summers of 1802, 1803, and 1806.

in doors and out, in the sun and in the shade.

Two of these thermometers, one mercurial the other spirit, were placed out of doors in the shade, and facing the north; two others were exposed to the direct rays of the sun; and two were within doors. All of them were constructed with the greatest care, and under the inspection of different members of the Academy of Sciences. Before Mr. Cotte examined the effects of different exposures, he determined, by taking the mean of a great number of observations, the comparative motions of these thermometers in the same situation.

Differences between mercury and spirit greatest in the sun.

It follows from these experiments, that the differences between the mercurial and spirit thermometers are much more considerable, when they are exposed directly to the rays of the sun; which Mr. Cotte ascribes chiefly to the red colour of the spirit: and this difference is greater, the greater the heat.

Hourly variations.

The greatest hourly variation takes place from 6 to 7, and more especially from 7 to 8 in the morning; it continues diminishing till 11; thence it increases till 2; and between 2 and 3 it diminishes a little.

The

The difference between the mercurial and spirit thermometers exposed to the sun is nearly the same from 10 in the morning till 4 in the afternoon.

The greatest heat without doors not always on the same days as within.

The maximum of the thermometers within doors does not happen on the same days as that of the thermometers without.

Clouds affect spirit most.

A cloud passing rapidly over the sun suddenly sinks the spirit 2° or 3°, the mercury about 1° only. When the cloud has passed, the liquid rises as quickly.

Mercury most uniform.

The motion of the mercury is most uniform.

Time of greatest heat.

The maximum of the thermometers out of doors in the shade takes place from 2 to 3: that of the thermometers in the sun, from 3 to 4: and that of the thermometers within doors, from 6 to 7, in the afternoon.

Fluctuation observed.

When the heat is the greatest, a kind of fluctuation and agitation is observed in the mercury, and still more in the spirit, which causes them to rise and fall continually.

Relation between the distances of any five points in space, by Carnot.

Mr. Carnot has published a memoir on the Relation that exists between the distances of any five points taken in space, followed by an essay on the Theory of Transversals. This forms an interesting appendage to the Geometry of Position of the same author. In it will be found a number of useful or at least very curious theorems; analytical formulæ for resolving all the problems respecting a quadrangular pyramid, without supposing any knowledge but that of its edges. All these formulæ are symmetrical, and possess a degree of elegance, that will much please the geometrician. Some, it is true, may stagger the hardiest calculator, and much shorter solutions might be obtained by the skilful application of trigonometry; but each problem would require new considerations, which do not immediately present themselves to the mind, while here every thing flows in the clearest manner from a few known principles. This work therefore is a repository, whence the geometrician may derive expressions, that will facilitate the solution of very complicated problems. To give an idea of the calculations of the author, we shall quote the enunciation of one of the last problems, which is as it were the summary of those that precede: "Of ten right lines, joining any five points taken in space two by two, nine being given to find the tenth."

The

The Essay on Transversals is not less curious. The fundamental principle of this likewise may be found in the Geometry of Position; and it was one of the two, on which Ptolemy built all his spherical trigonometry. The word transversal is here employed to signify any right line, cutting the three sides of a right-lined triangle or their prolongations. An equation of remarkable simplicity expresses the ratio between the segments of the sides. Mr. Carnot immediately deduces from it three other formulæ of the same nature, which, transferred afterward to spherical trigonometry, are found to be the same as Ptolemy had deemed sufficient for the purposes of astronomy. He demonstrated them synthetically, according to the method of the ancients; and his demonstrations, enlarged by his commentator Theon, are not very complex. Mr. Carnot, after having demonstrated the first principle exactly in the same manner as Ptolemy, finds in our modern trigonometry more simple means for the others. Carnot's Essay on Transversals.

After having coincided with the Greek mathematician, he extends the theory in various ways, applying it to plane and spherical quadrangular figures; to every polygon, plane or even oblique; and lastly to pyramids: applications that are perfectly new, and of which not the least trace is to be found either in Ptolemy, or in his commentator.

Mr. Lacroix has published a fifth edition of his Elements of Geometry. 5th edition of Elements of Geometry by Lacroix.

Mr. Haüy has published a second of his Elements of Natural Philosophy. The great and rapid success of the first edition renders it unnecessary for us to enter particularly into the plan and execution of a work, which its author has revised throughout, to enrich it with all the new discoveries, that have taken place in such a short interval. Thus we find in it Mr. Laplace's theory of capillary phenomena; Mr. Gay-Lussac's experiments on the dilatation of gases; and the researches of Mr. Biot into the relation between the refractive power of different substances and their chemical composition, which he has just finished. 2nd edition of Hauy's Elements of Natural Philosophy.

Lectures

Lectures at St. Thomas's and Guy's Hospitals.

Medical and surgical lectures.

THE autumnal course of lectures at these hospitals, will commence as follows:

St. Thomas's.

Anatomy and the operations of surgery, by Mr. Cline and Mr. Astley Cooper, Thursday, Oct. 1st, at 2 o'clock.

Principles and practice of surgery, by Mr. Astley Cooper, Monday, October 5th, at 8 in the evening.

Guy's.

Practice of medicine, by Dr. Babington and Dr. Curry, Friday, October 2, at 10 o'clock.

Chemistry by Dr. Babington, Dr. Marcet, and Mr. Allen, Saturday, October 3, at 10 o'clock.

Midwifery and diseases peculiar to women and children, by Dr. Haighton, Monday, Oct. 5, at 8 in the morning.

Pathology, therapeutics, and materia medica, by Dr. Curry, and Dr. Cholmeley, Tuesday, October 6, at 8 in the evening.

Physiology, or laws of the animal Œconomy, by Dr. Haighton, Wednesday, October 7, at 7 in the evening.

Experimental Philosophy, by Mr. Allen, to begin in November.

Clinical Lectures on select medical cases, by Dr. Babington, Dr. Curry, and Dr. Marcet.

N. B. The several lectures are so arranged as not to interfere with each other in the hours of attendance; and the whole is calculated to form a complete course of medical and surgical instruction. Terms and other particulars to be learnt from Mr. Stocker, apothecary to Guy's Hospital, who is also empowered to enter gentlemen as pupils to such of the lectures as are delivered at Guy's.

Fourcroy's Philosophy of Chemistry.

A. F. FOURCROY, professor of chemistry at Paris, has published an enlarged edition of his "Philosophy of Chemistry," which is considered as the best elementary work on that science. A translation of it by Mr. W. Desmond, is in the press, and may be expected early in September.

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END OF THE SEVENTEENTH VOLUME

Printed by W. Stratford, Crown-Court, Temple-Bar

A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

VOL. XVIII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR; FOR

THE AUTHOR,

AND SOLD BY

J. STRATFORD, No. 112, HOLBORN-HILL.

1807.

PREFACE.

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Capillary Actions of Fluids.

Fig. 1.

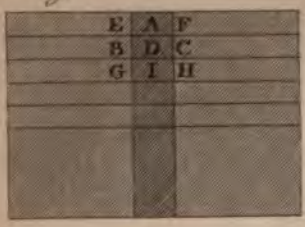


Fig. 2.



Fig. 3.

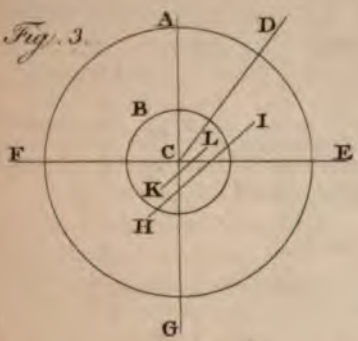


Fig. 4.



Fig. 5.



Fig. 6.

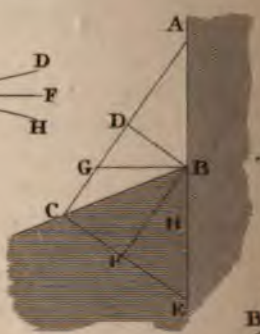


Fig. 7.

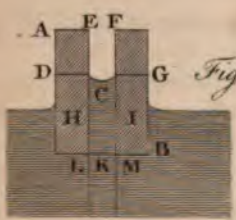


Fig. 8.

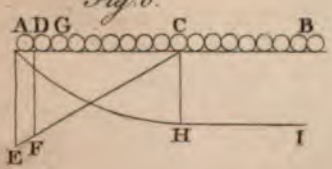
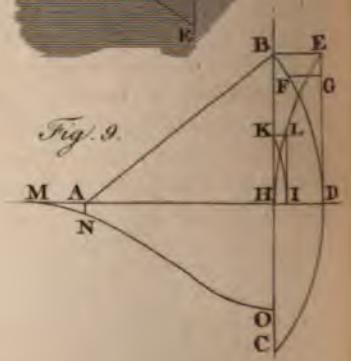


Fig. 9.



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1807.

ARTICLE I.

Remarks on some Difficulties which occur in the Investigation of the Capillary Actions of Fluids.

To Mr. NICHOLSON.

SIR,

THE capillary actions of liquids have lately been minutely investigated, both in this country and in France, and several essays on the subject have been inserted in your Journal: but there appears to me to be still some deficiency in all the modes of demonstration which have been employed. Mr. Laplace's first method leads to erroneous conclusions, respecting the angle of contact of a solid and a fluid: his second is less exceptionable, but it is still defective in omitting the consideration of the force of repulsion; for it cannot be denied, that this force is equally indispensable with that of cohesion to the existence of all material bodies in the state of solids or of liquids; and every theory of the mutual actions of the particles of such bodies, which does not comprehend the consideration of both these forces, must necessarily be imperfect. Dr. T. Young's reasoning, although built on more probable suppositions respecting the mutual actions of the particles, does not seem to be mathematically

Capillary action of fluids not yet completely investigated.

Laplace defective.

Force of repulsion necessary to be considered.

Dr. T. Young.

matically conclusive, so far as it relates to the physical foundation of the general law of an equal tension of the surface of any given liquid.

In the first place it appears, that Mr. Laplace's conclusion respecting the attraction, which he supposes to be exerted by a liquid, terminated by a plane surface, on any imaginary column within it, may be confuted on every supposition that can be formed, respecting the nature of the forces concerned. For the force which tends to draw every such column downwards into the liquid, can only be derived from the actions of the neighbouring columns, and must therefore tend in an equal degree to elevate them: so that the parts of each column which are nearest to the surface are urged downwards, and the remoter parts upwards, by equal forces; and the result is merely a general attraction of the whole stratum for the stratum next below, which of course must be completely counteracted by the repulsive force, whatever its nature may be. Thus the portion A (Pl. I. Fig. 1) is urged downwards by the attractions of the portions B and C, while D is urged upwards by those of E and F; and in the same manner D is urged downwards by G and H, while I is urged upwards by B and C. And thus, by continually adding to the substance any number of successive strata, we shall still find, that the general effect of the whole body on the column A I will retain it in equilibrium, whatever may be the depth. It is true, that according to Mr. Laplace's own principles, the attraction of any limited number of strata, on a column passing through them, must disappear, the force of the lower surface, which he supposes to be directed upwards, counteracting that of the upper in a contrary direction; but this consideration, although it may lead to a correct result with respect to the actions of fluids only, is not applicable to those of solids, or to the mutual actions of solids and fluids.

Force that tends to draw a column of fluid downward, tends to draw the neighbouring columns upward.

Counteracted by the repulsive force.

Thus equilibrium maintained to any depth.

Laplace's principles not applicable to solids, or solids and fluids.

In the second place, Mr. Laplace's determination of the attractive powers of a wedge of any kind, as proportional to its chord, must necessarily lead, as Dr. Young has already observed, to a proposition respecting the equilibrium of the surface of a fluid with a solid, which Mr. Laplace will not justify, and which he has silently abandoned; that is, that

His determination of the attraction of a wedge leads to erroneous principles.

that no angular termination of a fluid in contact with a solid can remain at rest, unless the density of the solid be precisely half that of the fluid. Thus if A B (Fig. 2) be the surface of a fluid retained in a horizontal situation by the vertical force C D, resulting from the joint actions C E and C F of the wedges A C H and B C H, if we add a wedge B C I opposite and similar to A C H, we shall have a straight line H I, and the action C K of this wedge reducing C E to C L, that of the wedge B C H must be reduced in the same proportion, in order that the result may remain in the direction C D, and the density of B C H must be made equal to the difference of the densities of B C I and A C H; or, if H I be the termination of a single solid, that solid must be of half the density of the fluid. It was perhaps in order to avoid this inference from his first theory, that Mr. Laplace adopted afterwards a different mode of reasoning.

Angular termination of a fluid in contact with a solid cannot remain at rest, unless the density of the solid be to that of the fluid exactly as 1 to 2.

I shall now examine the consequences of the supposition of a repulsive force extending its action to all particles within a certain very small distance of each other. Since it is certain, that the particles of all bodies in the state of gas repel each other, without any thing like the actual contact of impenetrable atoms; and since it may be shown by experiment, that many solid bodies exert repulsive powers on each other at sensible distances; it is natural to imagine, that the repulsive force, acting on any given particle, is derived from the joint effect of a considerable number of other particles at different distances from it, in the same manner as the force of cohesion is conceived to be derived from the joint actions of a great number of particles cooperating with each other; although the repulsive force may naturally be supposed, to consist principally in the stronger action of a smaller number of particles. Now if the circle A (Fig. 3) represent the limits of the force of cohesion, and B those of the force of repulsion acting on the central particle C, it is evident, that, if the substance be divided into any number of wedges meeting in the point C, the two forces exerted by any one of these D C E, upon any other F C G, must be equal, since the segments are in the same proportions as the whole circles; and the effects of the whole circles are

Consequences of a repulsive force.

Particles of gas repel at sensible distances, as do many solids,

therefore repulsion the joint effect of many particles,

and probably from the stronger action of fewer particles than operate in cohesion.

This applied to sectors of circles.

equal: for, if an imaginary separation be made in the substance in any direction A C, it is evident, that the cohesive force, tending to bring all the particles of the two segments together, must be equal to the repulsive force, which prevents their nearer approach; and, into whatever portions the cohesive forces of the wedges be supposed to be divided, it is obvious, that for each of these, as for example, the mutual actions of the particles situate at H and I, an equivalent may be found in the repulsive force K L, exerted between the particles which are similarly situate within the sphere of repulsion: consequently, the whole result must be not only equal, but also parallel: so that, if the wedge F C G be considered as the termination of a vertical column, the effect of the wedge D C E, or of D C G, will have no tendency either to elevate or to depress that column. The only want of perfect counteraction will be, that the parts nearest the wedge will be urged more downwards by the repulsive force, and the remoter parts more upwards by the cohesive force. In order to understand the effect of a combination of such actions where the surface is curved, let us suppose the superficial particles to be situate at the angles of a polygon, A B C D E F G H, (Fig. 4) and the repulsive force to extend only to the two nearest particles, one on each side, while the cohesive force is so distributed, as to have its general result directed to the next particle but one: it will then be necessary, in order that there may be an equilibrium between the forces tending to separate and to unite any two particles D and E in the direction of the surface, that the cohesive forces in the directions D F, E C, be represented by D I and I K, while D E represents the repulsive force: then the forces acting on D being represented by C D, E D, L D, and I D, it is evident, that the parts of these forces which tend to urge the particle D to and from the line C E, are precisely equal, so that this particle will remain perfectly in equilibrium, without occasioning any pressure on the stratum within it. It is supposed in Dr. Young's reasoning on this subject, that the repulsive and cohesive forces acting on each particle are either accurately or very nearly equal; but this supposition, although it appears

: counteron not abtely per-

ct of a bination of a actions.

Young's position.

pears at first sight unexceptionable, is in fact inconsistent with the general principles of the theory.

It appears therefore on one hand, that the consideration of a repulsive force is indispensably necessary to the perfect solution of the problem; and on the other, that such a force as there is reason to infer from experiment is not capable of producing effects similar to those of the capillary affections of liquids. There appears to be only one way of avoiding these difficulties, which is, to suppose that a *part of the force of repulsion* only is concerned in that action which is observed at sensible distances, while another part is so confined to the particles in immediate contact with each other, that if we suppose a liquid to be divided by any imaginary plane surface, the particles on one side of this surface can only act on the particles on the other side in a direction perpendicular to it, leaving them completely at liberty to move without resistance in the direction of the surface itself. This hypothesis has been tacitly assumed by Mr. Laplace, with respect to *the whole* force of repulsion; but in any shape it is still an *hypothesis* only, and the reasoning founded on it can only be considered as demonstrative, so far as its results are justified by a coincidence with facts and experiments.

Repulsive force therefore necessary to the solution; but the force deducible from experiment not adequate to the effect.

Part of this force only supposed to act at sensible distances.

Suppose a column or stratum A B (Fig. 5) terminating in a curved surface C D, to be contained between two parallel planes perpendicular to the tangent E F; then the action of the particles below E F will have no power to move the column in a vertical direction; but the portion of the substance included between the curve and its tangent will tend to elevate it, and the more in proportion as the curvature is greater; the number of particles within a very minute distance from the column being directly as the curvature, or, where the surface has a double curvature, as the sum of the two curvatures in directions perpendicular to each other. And if the line G H be every where as much below E F as C D is above it, the action of the particles, cut off by this line on the column A B, will be equal to that of the particles above E F, and will produce an equal force tending to raise it; hence, if all these particles be removed, the remaining parts of the substance below G H will attract the column with the same force as was before counteracted by

Supposition of a column terminating in a concave surface.

Surface of double curvature

Pressure proportional to the curvature.

by that of the parts removed; and the pressure will therefore in every case be proportional to the curvature.

A drop of fluid being perfecty insulated,

Hence if we imagine a drop of a fluid to be perfectly insulated, it is evident, that the superficial parts on one side of the drop must press the included fluid towards the other side, and must consequently be pressed back in an equal degree, so that at the circumference of the circle supposed to divide the drop, the surface must be *stretched* by the whole of this force, reduced only to a single direction; and there must therefore be *a uniform tension* of the surface.

the tension of its surface will be uniform

Exception where the surface of the fluid is bounded on each side by a solid of half the density.

The only case which can be supposed to afford an exception to this demonstration, is that of the surface of a liquid terminated on each side by a solid of precisely half the density: but it is of little consequence what may be the result of such a combination, since it is scarcely possible, that it should ever be observed in nature. If it were not true, that the surfaces of liquids are stretched by a uniform force, it would follow, that a cork, wetted on one side and greased on the other, would continue for ever to move, on the surface of a large reservoir, towards the wetted side.

Tension of fluid surfaces must be equable.

Hence the angle of contact of a solid and a fluid may be deduced.

The angle of contact of a solid and a fluid, of given density, may be deduced from the law of equable tension, when once established, in a very satisfactory manner. Conceive a body, of the density of the solid only, to extend through the substance of the solid and fluid A B C (Fig. 6); the attraction of its surface will then urge the angular particle in the direction B D, with a force which is to the whole tension as B D to half A B; then a substance equal in density to the difference of the solid and the fluid, being superadded to the wedge C B E, will draw the particle in the direction B F with a force B F: now in order that the forces in the directions B D and B F may produce a result B G, capable of being completely counteracted by the perpendicular attraction of the surface A E, they must be proportional to B D and D G, and the density of the additional portion C B E must be to that of the solid as D G to B F, or to D C or A D, and the whole density of the liquid C B E to that of the solid as A G to A D, that is, by similar triangles, as A E to A H, which is the versed sine of the angle A B C, A E being the diameter.

Mr.

Laplace's second method ingenious, but requires the same hypothesis of repulsion

Mr. Laplace's second method of considering the effects of capillary action, though not wholly new, is ingenious and satisfactory; but it requires the assistance of the same hypothesis respecting repulsion, as is necessary to his first theory. The attraction of a capillary tube A B (Fig. 7) on the column C consists of two equal parts, one of which is derived from the action of the part D E F G on the upper portion of the fluid at C, the other from that of the end of the tube at H I upon the portion below at K; and these two forces are opposed by the attraction of L M, the part of the fluid forming a continuation of the solid, which draws the column downwards in the same manner as each of the other forces draws it upwards: so that the weight of the fluid elevated must be proportional to the excess of twice the density of the solid above that of the fluid. Supposing the fluid to be elevated in a very narrow space of a given breadth, the half of this breadth being the radius, the secant of the angle of contact will become equal to the radius of curvature of the surface, which is always inversely as the height of the elevated column; hence the cosine of the angle of contact will be directly as the height, that is, as the difference between the density of the fluid and twice that of the solid, the whole density of the fluid being represented by the radius; and this determination agrees precisely with the former.

Laplace's observation on unequal elevations and depressions of a fluid round floating bodies just.

Mr. Laplace has very justly observed, that where two floating bodies are surrounded by an elevation and a depression which are unequal in height, their repulsion will become a maximum at a certain distance, and upon a still nearer approach will be changed into an attraction. When the distance is very small, the height of the fluid elevated between the bodies is the mean of the heights to which it would be raised between two similar portions of the respective substances, and hence the magnitude of the force may be readily determined. Dr. Young seems to have considered only the case of an equal depression and elevation.

This not considered by Dr. Young

Combined effects of cohesion and repulsion in the constitution of natural bodies,

As an illustration of the combined effects of the forces of cohesion and repulsion in the constitution of natural bodies, I shall subjoin a short investigation of the magnitude of the attractive power which retains the particles in solids and fluids

tural bodies.

fluids in their situation, upon the simple, although perhaps inadequate supposition of a congeries of incompressible particles in contact with each other, actuated by a cohesive force, which extends, without diminution of its intensity, to a certain small distance from each particle.

In the series of single particles A B (Fig. 8) the particle A, being attracted by all the particles between A and C, the limit of the cohesive force, presses on the next particle D with the whole of this force, which may be represented by the line A E: but the pressure occasioned by the cohesion of the particle D is only proportional to the line D F, which is to A E as D C to A C, because the mutual action of D and A takes from the whole cohesive force a part which is equivalent to the action of the particle next beyond C: hence D presses on G with a force represented by the sum of A E and D F; and in the same manner it may be shown, that the whole mutual pressure of the particles at or beyond C is expressed by the area of the triangle A E C; and in general, that it may every where be represented by the ordinates of the parabolic curve A H, or of the mixtilinear figure A H I. The same may be inferred from considering the whole force resisting the division of the series between any two of its particles.

Magnitude of the force of attraction.

Suppose now that a single particle A (Fig. 9) is placed beyond the limit B C of an attractive body; it is required to determine the magnitude of the whole force with which it is attracted. The force of the particles situate upon the arc B D, when reduced to the direction A D, is represented by the line D E, since the number of particles in any small portion B F is as much greater than in E G, as A H is less than A B; and in the same manner the force of the particles in the line or narrow ring I K is represented by the line I L; hence the attraction of the whole segment B D H will be represented by the area D H E, I L being always equal to H K, and the curve H E being a hyperbola, which, when A comes into contact with H, becomes a right line. But when B C is considered as representing the surface of a solid, the measure of the attraction is the hyperbolic conoid, or the cone described by the revolution of the line H E on H D as an axis; and hence the attraction of the solid on a particle

particle at H is precisely half as great, as if all the particles within the hemisphere of cohesion were situate in the line H D. If we take H M = A D, and make the ordinate A N' every where proportional to the content of the conoid corresponding to the distance A H, the curve M N will approach at M infinitely near to a parabola, and at O will become parallel to A H; and the area A M N will express the sum of the attractions of a series of particles extending from M to A, and consequently the proportional attractive force of two solids situate at the distance A H. It is easy to show, by a fluxional calculation, that the area H M O is half of the rectangle M H O, and consequently, that the mutual action of the substances when in contact, is half as great, as if all the particles of the one body within reach of the cohesive force of the other, were situate immediately at its surface.

If one of the bodies be equal in thickness to the distance to which the cohesion extends, it will still be attracted by the whole force of the other: but if its thickness be less, and equal, for example, to A H only, the attraction will be expressed by the area A H O N only, which is ultimately to the whole area H M O as twice A H to M H. This is perhaps the reason, why the superficial particles of liquids are easily detached by the action of heat, in the process of slow evaporation.

This accounts for the process of slow evaporation.

I am, SIR,

Your very obedient servant,

20 *July*, 1807. ALETES.

II.

On the Solubility of some of the Earths by means of Sugar. By *Mr.* WILLIAM RAMSAY.

SIR, *Glasgow, July* 14, 1807.

SHOULD you think the following experiments, on the solubility of some of the earths by means of sugar, worthy of

of notice, you are at liberty to publish them in your valuable Journal.

I am, SIR,

Your most humble servant,

WILLIAM RAMSAY.

WM. NICHOLSON, Esq.

London.

Quicklime gave a caustic taste to solution of sugar.

BEING employed in making experiments on sugar, and happening to put some quicklime into a cold solution of it, I noticed, that it had acquired an uncommon caustic taste.

Sugar dissolved in limewater is different.

Uncertain whether sugar dissolved in common lime water might not have the same taste, I prepared a small quantity, and added sugar to it; but the taste of the solution was very little different from that of sugar dissolved in water. On adding diluted sulphuric acid to the former, a copious precipitation of sulphate of lime took place, while the latter scarcely showed any traces of the presence of lime by the same agent.

Sugar dissolves lime.

Hence I concluded, that sugar possesses the property of dissolving a certain proportion of lime; and in order to ascertain its capacity in this respect, the following experiments were made upon this earth, together with barites, strontites, magnesia, &c.

Solution of sugar, at 50°,

One pound avoirdupois of common unrefined sugar was dissolved in rain water, and the solution filtered; the specific gravity at the temperature of 50 degrees of Fahrenheit's thermometer was found to be 1040. This solution was used in all the following experiments as a standard, to which the earths were added at first at the temperature of 50 degrees.

dissolves lime in

A portion of the saccharine solution was taken, quicklime was added to it in superabundance, repeatedly filtered, and fresh portions of the earth given to it, until the solution was evidently saturated; when the specific gravity was found to be

Solution of sugar	1020
Increased sp. grav. from lime in solution	40
	1060

Consequently

Consequently sugar dissolved in water at the temperature of 50 degrees is capable of dissolving one half of its weight of lime. the proportion of 1 part to 2 of sugar.

As most salts combine with greater facility, and in general in larger proportion by the medium of heat, it was thonght, that the action of the sugar on the lime might be increased, and a greater quantity of it dissolved, at a higher temperature. Fresh quicklime was boiled in the solution of sugar. On filtering and cooling the liquid, it was found, that very little of the earth was dissolved. On the addition of dilute sulphuric acid, a slight cloudiness only appeared; but by the addition of oxalic acid to another portion of the liquid, a precipitation of oxalate of lime took place, which was estimated at about twice the quantity of lime that would have been precipitated from common limewater by the same agent. Heat does not much increase its solvent power.

The solution of lime in sugar is of a beautiful white wine colour, and has the smell of fresh slacked quicklime. Chemical properties.

It is precipitated from the solution, by the carbonic, citric, tartarous, sulphuric, and oxalic acids. And it is decomposed by double affinity, by caustic and carbonated potash and soda, the citrate, tartrite, and oxalate of potash, &c.

Having ascertained the quantity of lime that is dissolved in a given quantity of sugar, I next tried it as a solvent of strontites. Two ounces of this earth were taken, and the carbonic acid expelled by dilute nitric acid; the mixture was evaporated to dryness, and then put into a crucible in a red heat, until the nitric acid was decomposed. A portion of the solution of sugar was taken, and the earth added to it in the cold state; the specific gravity was increased to 1050. This solution was boiled on a fresh portion of earth, and the liquid immediately filtered; for some time it remained of a pure white wine colour, but as the liquid cooled, it gradually deposited a number of gray coloured crystals, which are soluble in water, and have the same taste as the saccharine solution of strontites. At the temperature of 50 degrees, the specific gravity of this solution was as under. Sugar in solution dissolves 1-4th of strontites at 50°, and an equal weight at 212°, which it retains at 50°.

Solution of sugar	1040
Increased sp. grav. from strontites in solution	40
	1080

Consequently

Consequently an equal weight of strontites with the su employed is capable of being dissolved at the tempera of 212 degrees; and of being retained in solution by sugar at 50 degrees of Fahrenheit. On exposing the cr tals, which had fallen down during the cooling of the liq to the air of the atmosphere, they attracted carbonic a and effloresced.

Chemical properties.

The solution of strontites in sugar is of a fine white colour, and like that of lime has a peculiar caustic ta This earth is precipitated by caustic and carbonated pot and soda; also by the carbonic, citric, tartarous, sulphu and oxalic acids. And it is decomposed, by compound finity, by the carbonates of potash and soda; also by citrate, tartrite, and oxalate of potash.

Sugar and barytes.

Judging from the greater solubility of strontites w compared with lime in the saccharine solution, that might proceed from its superior affinity for this substa it was thought, that a greater proportion of barytes wo be dissolved than of either of the former earths. T ounces of the carbonate of barytes were taken, and tre in the same manner as has been narrated in the prepara of the strontian earth, by expelling the carbonic acid dilute nitric acid, evaporating to dryness, and then igni the mixture until the nitric acid was destroyed. The pared earth was put into the saccharine solution in the state, and frequently agitated; the liquid assumed a greenish appearance, and the smell of carbonated hydr gas was sensibly felt. After 24 hours rest the solution had its green colour, and was of the same colour as the ori

None dissolved cold,

solution of sugar; and a black matter was found precipit to the bottom of the glass jar. On trying the specific vity of the solution it was not increased. The liquid then taken and boiled on a fresh portion of the barytic e

and but little hot,

then filtered; on being cooled to the temperature of 5 grees, the specific gravity was

Solution of sugar	10
Barytes in solution	
	10
	1

rom the result of this experiment being so very different The experiment re
those preceding it on lime and strontites, I supposed, some change had taken place either in the sugar or bar-
; the experiment was therefore repeated several times, always with the same result. The barytic earth, which The baryt acquired c bonic acid.
left on the filtering paper, was put into dilute nitric acid, a violent disengagement of carbonic acid gas took place, ough the whole of this gas was apparently expelled be-the earth was introduced into the saccharine solution. same portion of earth was treated in the same manner Repeated with the same earth.
s, and on expelling the carbonic acid and introducing to the saccharine liquor, the specific gravity was not ir increased; the liquid always assumed the greenish arance before noticed, and when in this state carbonated gen gas was evidently disengaged, and a black floccu-matter always subsided before the liquid became trans-nt.

om these appearances one would be almost ready to Probably the barytes decomposed the sugar by its affinity to carbon.
nde, "That barytes, by its superior affinity with the bon of the sugar, is capable of decomposing it; that rt of the carbon, in union with the barytes, is precipi-ed along with the earth in its carbonated state; and o the oxigen of the sugar, being set at liberty, unites h the hidrogen and another portion of the carbon, and disengaged in the state of carbonated hidrogen gas." s I cannot say, that the earth and the nitric acid were tate of absolute purity, on this account I dare not trust ly to this explanation, and only state what took place g the course of making these experiments.

ext tried the power of the solution of sugar on mag- Magnesia very little soluble in it.
. One half ounce of this earth calcined was added to ccharine solution in the cold state, without the specific y being perceptibly increased; the mixture was then , when on filtering and cooling the liquid to 50 degrees, ecific gravity was

ution of sugar	1040.000
gnesia in solution	3.050
	1043.050

The

Alumine but little soluble in sugar.

Very little alumine is dissolved by a solu when fresh precipitated earth is presented to it cold or hot state. Neither does it seem capal it in solution, when sulphate of alumine is de saccharite of lime in the way of double decom the lime and alumine are precipitated together fresh precipitated earth of alum is boiled for the saccharine solution, it seems capable of colouring matter of the sugar, and the liquid, v has precipitated, is in a purer state than bef with certain modifications this agent might be the refining of sugar.

Perhaps it might be useful in refining it.

Alkaline carbonates separate the earth from sugar.

The union of sugar with the alkalis has been but this is rendered more strikingly evident, b potash or soda (for instance) decomposing th lime and strontites in sugar by double affinity.

Weak sugars contain lime.

In making solutions of unrefined sugar for poses, a gray coloured substance is found freq pitated. It is probable, that this proceeds f abundance of lime, which has been used in juice of the sugar cane at the plantations ab with this imperfection is known among the re article by the name of *weak*. And it is just the precipitated matter being nothing but lir attracted carbonic acid from the sugar, (of wl great probability) or from the air of the atı bottle in which I had kept a solution of lime i

Lime separates from sugar in

III.

Inquiries concerning the assimilating Power in Vegetables; by Mr. HENRY BRACONNOT: *read at the Academical Society of Sciences of Nanci, November the 22d,* 1806*.

Supposed that vegetables extract nutritious juices from the earth, refuted by Van Helmont, Boyle, and others.

PHYTOLOGISTS for a long time imagined, that vegetables were nourished by certain juices, which they extracted ready formed from the earth. Van Helmont in great measure refuted this by his celebrated experiment. In a box containing 100 lbs. of earth, and covered with lead, he planted a willow, weighing 50 lbs. This he watered with distilled water, and in five years it had acquired an addition to its weight of 119 lbs. 3 oz. without any perceptible diminution of the earth. The experiments of Boyle with earth baked in an oven, and those of Duhamel and Bonnet with moss†, prove the same thing.

Tillet showed air and water alone to be necessary to their growth.

Other natural philosophers have pursued the same inquiry: Tillet, in particular, made a number of experiments, to ascertain whether water and air were the only substances necessary for the growth of plants. He filled several pots with different earthy matters, some with old plaster, others with pure river sand, fragments of stone broken to powder, &c.; buried them partly in the ground, to retain the moisture; and sowed wheat in them. The wheat produced very fine ears; and the grains, being sown, produced other fine plants.

None of these plants analysed

From the infant state of chemistry, at the time, however, none of the plants produced by means of air and water alone were analysed. This indeed has since been done; and it has been advanced, that plants growing in such a manner as to have been nourished by water alone, did not furnish as much

Since said, that these plants contained less

* Abridged from the Annales de Chemie, Vol. LXI. p. 187. Feb. 1807.

Seeds most difficult to germinate succeed in moss.

† Mr. Procopius Densidoff of Moscow, who has a very fine botanic garden, raises all sorts of plants by a peculiar method. He sows the seeds in moss, where they germinate, and then plants them in pots. In this way he loses very few seeds of those that grow with most difficulty. *Note of Prof. Willmett.*

carbon

1 than seeds.

carbon as was contained in the seeds from which they sprung; while those in mould were much more vigorous, in consequence of the carbon with which it furnished their roots. But these experiments were on too small a scale to furnish satisfactory results; and I have therefore attempted to investigate the subject anew, in order to ascertain, how far this opinion is well founded.

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As a preliminary step, I conceived it necessary to analyse vegetable mould in a state of perfect decomposition. For this purpose I selected perfectly pure, black, pulverulent mould, from among the hollow roots of a very old tree.

ıtained no- ng soluble water.

Distilled water, in which it was boiled, remained colourless after filtration, and on being evaporated left no sign of any soluble matter.

Very retentive of moisture.

A hundred grammes [3½ oz.] were reduced by dessication to 20, which shows it to be extremely retentive of water.

Its products.

These 20 gr., distilled at a red heat, gave out 4 of water, that powerfully reddened infusion of litmus; and contained empyreumatic acetous acid, partly saturated with ammonia; 2 of an acrid oil, that congealed on cooling, and was of a dark brown colour; 89 cubic inches of oily hidrogen gas, burning with a blue flame; and 34 cubic inches of carbonic acid absorbed by lime.

Residuum.

The coally residuum weighed 8·5 grammes, which were reduced by incineration to 2·4 gr. of light yellow ashes.

Ashes.

Boiling distilled water digested on these ashes acquired no peculiar taste, did not turn sirup of violets green, and was barely rendered turbid by the addition of a few drops of oxalic acid, which seemed to indicate, that a few particles of lime had been set free by the calcination. The nitrates of barytes and of silver produced no change in it. On further analysis these ashes afforded 1·3 of a gr. of silex, ·2 of oxide of iron containing a little oxide of manganese, ·25 of phosphate of lime, ·2 of lime, and some traces of magnesia.

The mould boiled in solution of potash.

I boiled 6 gr. [92 grains] of the same mould for an hour, in a strong solution of caustic potash, when it became glutinous, and swelled up. I then diluted it with water filtered, and obtained a very deep brown liquor. This mixed with the lixiviating waters gave on the addition of an acid a precipitate, that weighed 1 gr. when dried. It was of a very

very black colour, and in little shining scales. Scarcely any vapour arose from it when thrown on burning coals, and I consider it as charcoal well saturated with hidrogen. Art may imitate this substance, by effecting by fire what nature does by time. If we deprive a vegetable substance of almost all its oxigen, and a small quantity of its hidrogen, by exposing it to a certain temperature, the result will be a hidroguretted charcoal, partly soluble in potash, as I have found by experience.

The residuum resembled pitcoal.

That part of the mould, which had resisted the action of potash, weighed when dried 5 gr. It had no longer the physical characters of mould; was in pieces that were tolerably hard and brittle; and had a striking resemblance to pitcoal, which led me to produce it in larger quantity. In this way it had still such a resemblance to coal, that I could compare it to nothing else.

It affords no soluble matter to plants.

From this examination of mould it appears, that it cannot supply plants with any soluble matter for their nutrition, since boiling water has no action on it. It would be superfluous to say, that seeds sowed in it vegetated with extraordinary vigour; but I must not omit to mention the presence of a large quantity of potash in the plants, though I could not detect any in the mould in which they grew, by the most strict researches.

Manure therefore seems to be of use only by retaining moisture.

These observations appear to corroborate the opinion of Tillet and Parmentier, who consider manure as useful only by retaining moisture, and keeping strong soils open for some time, so as to allow the roots of plants to penetrate them. But if water and air be the only sources of the food of plants, any insoluble matter, sufficient in quantity, and duly watered, must be adequate to the purpose of their growth. This I endeavoured to ascertain by experiments.

Mustard seed sown in litharge.

Exp. I. In a box filled with pure litharge I sowed 400 seeds of white mustard, weighing 2·2 gr. These I watered frequently and carefully with distilled water. The box was placed in a good aspect, and a glass was hung over it to keep out the dust. The plants throve very well, and produced perfect pods. I collected all the seminal leaves as well as the rest that dropped off; and when the vegetation was at its height, pulled up the plants. Having well washed

the roots, to remove any portions of oxide of lead, and wiped them dry, the whole weighed 264 gr. After they were dried, the weight was 34·2 gr.

The produce analysed.

These yielded 12 gr. of coal, which by incineration were reduced to 4·2 gr. of ashes. These lost by lixiviation 2·2 gr.; of which ·59 gr. were sulphate of potash, ·69 gr. potash. The insoluble residuum afforded ·4 gr. of silex; ·45 gr. of oxide of iron, alumine, and phosphate of lime, the proportions of which were not determined; ·57 gr. of oxide of iron; and a very small portion of magnesia.

Mustard seed sown in flowers of sulphur.

Exp. II. In a very large, deep, and perforated bowl of stone ware, filled with well washed flowers of sulphur, 250 seeds of white mustard were sown. The whole was covered with a large bell glass, allowing free access to the air and light. The plants grew vigorously, being frequently watered with distilled water; as sulphur, having little affinity with water, parts with it very easily, and dries on the surface. They produced flowers in tolerable abundance, and the seeds produced plants in common ground. The weight of the fresh plants, with the fallen leaves, was 108 gr.; and when dried 18·6 gr. Their coal weighed 7·8 gr. and left 1·55 gr. of whitish ashes, which afforded by lixiviation ·6 gr. of carbonate and sulphate of potash. The insoluble part was similar to that of the former.

The produce analysed.

Mustard seed sown in small shot.

Exp. III. A hundred seeds of white mustard were sown in twenty pounds of very small unglazed shot, on the 9th of July. On the 28th of August they began to flower, and afforded small pods. All these plants were slender, and had but few and small leaves. When fresh they weighed 10 gr. and after being dried 2·3 gr. they yielded very little coal, but more than the weight of the seed. The weight of the shot appeared to oppose too great an obstacle to the young roots, as most of them spread over the surface, without being able to penetrate it. The little affinity of the lead for water was another reason of the feeble growth of the plants; and hence I have found, that plants thrive less in metallic powders, than in their oxides.

Radish seed sown in a heap of sand.

Exp. IV. On a flat stone pavement a bed was formed, about a yard high, of fine sand, taken immediately from the bed of the river, and well washed. In this were sown seeds

seeds of the common radish *(raphanus sativus)*, which were frequently watered with perfectly pure rain water, and the plants grew with as much vigour as they would have done in any ground. The greater part of the radishes were brought to table, and were of a very delicate flavour, without any of the disagreeable earthy taste they have sometimes. Some of them were left to seed; and most of these grew to the height of 2 feet or 2½. It was observed, that those at the top of the heap were much larger and stronger than those near the bottom *.

Sixty three of these plants when dried weighed 372 gr. Incinerated they left 54·2 gr. of gray ashes. These afforded by lixiviation 18·6 of very fine potash. From this I am inclined to think, that the radish might be cultivated with advantage on wet sandy places by the sea shore, for the purpose of fabricating potash †. These 18·6 gr. being farther analysed,

The produce analysed.

* A skilful gardener informs me, that asparagus too will succeed very well in pure river sand. Potatoes also grow well in sand, and are said to be better tasted.

Asparagus and potatoes grow well in sand.

† It appears, that potash abounds in all the plants of the class tetradynamia, and the ashes of some of the species were long in use for making soap and glass, before the introduction of soda as an article of trade. According to Bomare, the bunias cakile, sea rocket, was much employed for these purposes.

Potash abounds in tetradynamic plants.

I must here add an observation, which appears to me pretty general, and which I made in examining the acrid and bitter properties of plants. One or other of these principles I have almost always found in conjunction with a large quantity of potash, which was frequently saturated with nitric acid. Thus among the cruciferous plants, which are all more or less acrid, the sisymbrium nasturtium, common water-cress, afforded me a great deal of alkaline matter after incineration; and when fresh I found in it nitrate of potash. I have observed the nitre melt on incinerating cabbages and turnips; and Mr. Delaville found this salt in large quantity in the sap of these plants. Mr Bouillon-Lagrange found a large quantity of potash in the ashes of the erigeron canadense, Canada fleabane, which is acrid. The ashes of tobacco, the acrimony of which is well known, yield 40 per cent of potash. Among the bitter plants I have examined, I found nitrate of potash in the fumitory, 100 parts of the ashes of which contain more than 36 soluble in water, according to Wiegleb and Rukert. The common centaury, marsh and Siberian trefoil, and different species of the genus centaurea, which are very bitter, afford ashes in which potash abounds. Whether in these plants it be

Acrid and bitter plants generally abound in potash, frequently combined with nitric acid.

 saturated

analysed, were found to contain 6·7 of pure potash; 7·35 of sulphate of potash; a small quantity of phosphate of lime; and the rest was carbonic acid.

Residuum contained sulphur.

The residuum left after lixiviating the ashes appeared to contain sulphur, as on pouring nitric acid over it sulphuretted hidrogen was given out; but I could not find any phosphoric acid in it. I did not examine it for the earths, as these might have been said to have been taken up from the sand.

Compared with other plants from garden mould.

Having thus examined these plants, I thought it might not be amiss to compare their produce with that of some others, which had grown in common garden mould. Of these dried 372 gr. afforded but 34 of ashes, which it is true were very saline, and yielded 16 gr. of saline matter, consisting of carbonate and sulphate of potash. In the incineration of these plants too I observed a very copious production of ammonia, on pouring a little water on their ashes while still hot.

Whence these substances found in plants nourished only with water?

Are they all formed ftom oxigen and hidrogen?

But whence come these earths, alkalis, acids, metals, sulphur, phosphorus, found in plants, that have had no aliment but pure water? Can vitality, in conjunction with light and heat, determine certain quantities of oxigen and hidrogen to form by peculiar condensations those substances which have been considered as simple? this might put us on examining in a new point of view all those substances, that chemistry has not yet been able to decompose, and thus perhaps the conjectures, that have been advanced by some, may be verified.

Is it the same with animals?

We may even extend these remarks to animalization, supported by the well-known experiment of Rondeletius, who

saturated with nitric acid I have not ascertained. I must observe, however, that I have found no nitric acid in wormwood, 100 parts of the ashes of which afford nearly 75 of saline matter, according to Wiegleb. This large quantity of alkali has appeared to me to be partly saturated with a peculiar matter, which is deposited by a decoction of the fresh plant, and may be precipitated abundantly by nitrate of lead. This matter dissolves very well in alkalies neutralizing part of their properties: it is the same that Mr. Vauquelin has found in some species of cinchona. Does exist in all bitter plants? and is it this which in cinchona and wormwood cures intermittent and low fevers?

The alkali partly saturated with a peculiar matter:

is it cinchonin?

kept

kept a fish in pure water, till it grew too large for the vessel containing it, and by other similar experiments on different animals. It would even seem, that food acts on the stomach in a great measure as mould does on the roots of plants, merely retaining water in such a state of division, as to fit it for absorption and assimilation.

Does food act on the stomach merely as mould on the roots of plants?

From what has been said it appears, that foreign matters dissolved in water only check the progress of vegetation; but that the vital powers can sometimes surmount these obstacles, appropriating only the pure water, that held these matters in solution.

Matters dissolved in water injurious to vegetation.

If experiments founded on long practice were still necessary to prove, that the soil is so much the more proper for vegetation in proportion as it is deprived of soluble foreign matter, I would mention the practice of paring and burning wastes, used chiefly in England. Lands thus treated remain in heart a long time; the parts where the heaps of surface mould were burned are most fertile; and manure even appears to be injurious, by causing the wheat to run chiefly to straw, with thin ears, and those of bad quality.

Absence of soluble matter advantageous, as appears from the effect of paring and burning;

This extraordinary effect of torrefaction on the soil appears to me attributable to the combustion of those excrementitious matters, which are ejected by the roots of plants. When the soil is impregnated with these matters, which are eminently injurious to vegetation, the perfect developement of plants is prevented. This excretion from the roots is evident from the surrounding earth, which becomes unctuous, and sometimes of a darker colour. In several of the euphorbiums and cicoraceous plants it is very perceptible, and milky. It may be observed too, that roots, when they multiply under water, become covered with a glairy matter, which deserves to be examined; and which no doubt the earth would have absorbed, had the roots remained buried in it. It is to these excretions from the roots perhaps we must frequently ascribe that kind of antipathy between certain plants, which are never found together. Thus the thistle is injurious to oats, euphorbium and scabious to flax, elecampane to carrots, fleabane and darnel to wheat, &c.

which probably acts by destroying the matter excretted from the roots of plants.

This excretion an obvious fact;

and probably the cause why some plants particularly injurious to others.

It would certainly be wrong, to ascribe the fertility of land pared and burned to the charcoal produced in this operation; for

Effects of paring and burning cannot be

owing to charcoal.

for Mr. Chaptal has shown, that dry charcoal, alone or mixed with earths of little solubility, does not penetrate into the vessels of vegetables.

Plants do not draw charcoal from the earth.

To add to the proofs, that vegetables have no need of drawing carbon from the earth, I might mention high trees, loaded with fruit, that grow and thrive on rocks or old walls, totally destitute of vegetable mould; and those vast forests, the soil of which is pure sand extending far beyond the roots.

Opinion that they derive it from the atmosphere.

I have now to examine the opinion, that vegetables absorb their carbon from the small quantity of carbonic acid contained in the astmosphere. Sennebier first announced this decomposition; and T. Saussure afterward endeavoured to prove, that this very small quantity would be sufficient for all the vegetables that exist. But though this philosopher was persuaded of the utility of carbonic acid in vegetation, he satisfied himself, that plants could grow in an atmosphere freed from it; and he ascribed this growth to the carbonic acid produced by the plants themselves, which they decomposed after having formed it*. To prove this he exposed to the sun closed receivers, in which plants were growing, and suspended quicklime to the upper part of them. The plants soon grew yellow, and at the expiration of five days gave no signs of vegetation; whence he inferred, that the absorption of carbonic acid by the lime was the cause of their death, and that the elaboration of this acid was necessary to vegetation in the sun. But I cannot be of his opinion. I have examined the experiment carefully, and satisfied myself, that the death of the plants was not owing to the privation of carbonic acid alone, but to the lime itself in vapour.

Saussure's experiment.

The plants killed by the vapour of the lime.

The following experiments convinced me of the volatility of lime.

Proofs that lime is in some measure volatile.

1. Paper tinged by repeated immersion in infusion of litmus, then reddened, and afterward washed in water to remove its excess of acid, was suspended in a stopped phial,

* It is obvious, that the carbonic acid formed by the plants could not furnish them with more of its base than it had previously taken from them. Tr.

into

into which I had put with great caution some lime, that was slaked, and suitably moistened with water. It was not long before the red colour of the paper was changed to blue. This effect was not unknown to Fourcroy.

Distilled with water.

2. Into a retort I put with all possible precaution a certain quantity of lime and water, and by distillation I obtained a liquor impregnated with an intolerable smell of lime. This liquor left a disagreeable impression on the palate, and had manifestly alkaline properties.

Still more rises with spirit.

Alcohol by its volatility carries up in vapour a much larger quantity of lime, as appears from an experiment of Proust. In order to obtain spirit free from acetic acid, he distilled 25lbs. of red wine with a handful of quicklime. The product was so much impregnated with the taste and smell of the lime, that he was surprised. When redistilled it had the same taste, precipitated metallic solutions and oxalic acid, and restored the blue of litmus.

Other fixed alkalis rise at a low heat.

Lime is not the only fixed alkali, that shows a disposition to rise at a pretty low temperature.

Potash distilled.

A solution of potash, subjected to distillation, afforded me a water with a strong lixivial smell. This water redistilled retained the same smell, and gave with nitrate of lead a white flocculent precipitate, which was completely soluble in distilled vinegar.

Other substances partially volatile.

But there are other substances beside alkalis, the volatility of which is so little apparent in the temperature of the atmosphere, that it is discoverable only from its effects on organized beings.

Quicksilver.

Some Dutch chemists set plants in water, by the side of which they placed a small bottle of mercury, and covered the whole with a jar standing in water. On the third day the plants were covered with black spots, and on the fourth, the fifth, or at latest the sixth, they were entirely black. The effects were the same when the jar rested on pieces of cork on a table. Other plants lived a long time under similar circumstances except the presence of mercury.

Other vapours injurious to vegetation.

Sennebier and Hubert too have shown, that the vapour of sulphuric ether prevents germination from taking place, without altering the quantity of the air. Camphor, oil of turpentine, assafetida, vinegar, ammonia, bodies in a state of

of putrefaction, &c., have the same effect. Hence we may infer, that all those matters, which are injurious to animals, sensibly affect vegetables likewise.

Carbonic acid not useful,

We cannot therefore lay much stress on Saussure's experiments to show the utility of carbonic acid in vegetation, particularly when we recollect an experiment of Priestley's, which proved, that an atmosphere with an eighth part of carbonic acid was sufficient to kill two plants of mint, though this small quantity of acid was in contact with a large surface of water.

but injurious in no very large proportion.

Seeds germinate in oxide of lead,

Having found by experiment, that seeds germinate very well in oxides of lead, which are known to be greedy of carbonic acid, I conceived, that these might contribute to elucidate the question respecting the utility of carbonic acid in vegetation. In consequence I moistened with distilled water some recently prepared oxide of lead in the first stage of oxidation. This mixture I introduced speedily into a flint glass bottle: and though the disagreeable and as it were alkaline smell that arose from it, led me to doubt the success of my experiment, I sowed some mustard seed in this oxide, and corked the bottle tight. As I foresaw, no germination took place: but what I was far from expecting, and to my great surprise, part of the oxide of lead in the water was reduced by the seeds, each of which was enveloped by a shining coat of metallic lead. This appeared to me to be very probably owing to a production of water by the union of the oxigen of the oxide with the large quantity of hidrogen, that is condensed in this oily seed, which after the reduction was more or less carbonized.

but not if recently prepared,

and included in close bottles.

The lead partly reduced.

If the oxide have been exposed to the air, the seeds grow.

If the oxide of lead be left exposed to the air for some time after it is made, and then put into a bottle with water and seeds, no reduction of the metal will be effected, but germination will take place.

Oxide of lead easily reduced unless carbonic acid prevent it.

These experiments show the extreme facility, with which the oxides of lead are reduced, and the obstacle that carbonic acid opposes to this reduction.

As these first attempts did not afford me the result I sought, I availed myself of an old experiment of Huyghens, who, in 1672, put some earth into a bottle, corked it up, and found it produce such a quantity of plants, as almost to fill the

Seeds sown in siliceous earth, in corked bottles,

the bottle, without having had any fresh air admitted to it. Accordingly I procured six large flint glass bottles, most of which were square: filled them in part with very fine white sand, which I deprived of all calcareous earth by washing with weak muriatic acid; and moistened this with distilled water. The remainder of the bottle was filled with atmospheric air freed from carbonic acid.

without the presence of carbonic acid.

In these bottles having sowed 460 seeds of white mustard, I closed them very accurately, and placed them a few inches deep in a moist soil. Vegetation soon commenced, and considerable verdure was produced*. After six weeks growth my plants were liberated from their prisons, washed with great care, and dried. In this state they weighed 9 gram. [140 grs.] I filled a phial with them, which terminated in a narrow tube, and exposed it gradually to a strong heat. Thus I obtained 4 gr. 8 dec. [74 grs.] of coal. But as I supposed this coal might still contain a little sand, I incinerated it, and found 3 gr. 3 dec. [51 grs.] of very alkaline ashes. Consequently there was $1\frac{1}{2}$ gr. [23 grs.] of pure carbon.

They grew,

and afforded carbon,

In a very small vessel I distilled 460 white mustard seeds, and from this highly hidrogenated seed I obtained only 4 gr. [62 grs.] of coal, which lost near half its weight by calcination. Hence it follows, that 460 mustard seeds acquired in close vessels upwards of a gramme [$15\frac{1}{2}$ grs.] of pure carbon,

in larger quantities than the seed.

* It may be supposed, that these seeds did not germinate with as much vigour, as if in the open air. This however I do not think must be ascribed to the want of oxigen; for by trial of the air with a sulphuret before and after the experiment, its proportions appeared to be nearly the same. This is agreeable to the experiments of Hassenfratz, who convinced himself, that plants do not diminish the quantity of oxigen in a confined atmosphere: and I am inclined to think, that oxigen acts on plants merely as a stimulant, which is not indispensable, for Homberg found different seeds germinate in the vacuum of an airpump. The principal cause, that prevents the complete developement of plants in close vessels, appears to me to be owing to their abundant perspiration, which throws out the excrementitious fluids, that are so fatal to them even in the open air, as to render a certain space among their neighbours necessary to their vigorous growth.

The plants grew weakly, but not from want of oxigen.

Probably from their perspiration not being carried off.

which

which appeared evidently to have been formed at the expense of water, and probably of light*.

Arguments from geology that the carbon of plants does not come from the atmosphere.

Geological facts too seem to shake that theory, which ascribes the carbon found in vegetables to the small quantity of carbonic acid contained in the atmosphere. How indeed can so small a portion of this acid, scarcely amounting to a ten thousandth part of the air, explain the formation of those vast mines of pitcoal, which still retains the marks of those organized substances whence it originated, and the organic origin of which is sufficiently announced by its composition of carbon, hidrogen, oxigen, and azote? But without appealing to these ancient productions of the vegetable kingdom, buried in the earth in such abundance, we need only cast an eye on its surface, to satisfy ourselves that nature must have taken other steps to produce carbon.

Charcoal produced from water, and therefore contains hidrogen.

On the other hand, if, in the silent progress of vegetation, the elements of water concur with the solar light to produce charcoal by intimate combinations unknown to us, charcoal ought to contain hidrogen likewise; and this is confirmed by experience.

Proofs of this.

If charcoal strongly calcined be urged in the fire with a substance that has an affinity for hidrogen, the charcoal is partly decomposed, and hidroguretted products are obtained.

Light necessary to the production of carbon in plants.

* To satisfy myself, that plants can appropriate to themselves the elements of water, so as to constitute their different materials, only by their own organic action combined with that of light, I caused a given quantity of seed to grow in complete darkness, and at the common temperature of the air. They shot out long white filaments, at the extremity of which were the two seminal leaves; but nothing more appeared. After desiccation these plants weighed less than the seeds whence they sprung: which appeared to be owing to their having lost carbon in this languishing state, instead of acquiring it.

Apparently it combines with them.

But the mode of action of light on vegetables remains yet to be known. It appears however, that it enters into combination with them, and that to this combination is owing the green colour of their leaves, and the variety of hues admired in their flowers. Yet Mr. Humboldt has found green plants growing in deep and dark mines, the atmosphere of which contained a great deal of hidrogen. Does not this fact indicate something common between hidrogen and light, particularly when we observe, that these two fluids, the lightest in nature, seem likewise to produce analogous effects on some metallic oxides and salts?

Perhaps analogous with hidrogen.

Mr.

Mr. Berthollet mixed 30 gr. [463 grs.] of charcoal calcined in a forge fire with 20 gr. [309 grs.] of sulphur, and by distillation in a porcelain retort obtained more than 100 cubic centim. [391 lines] of sulphuretted hidrogen gas: and it appears to me to be probable, that, if the experiment were frequently repeated with the same charcoal, it might be totally decomposed, a fact that it would be interesting to verify.

If oxigen in the state of gas be presented to the charcoal instead of sulphur, water is formed, as is proved by the experiments of Lavoisier on the combustion of charcoal, as well as by those of Mr. Hassenfratz: and analogous results are obtainable with metallic oxides, according to the observations of Cruikshank.

The diamond too contains hidrogen.

It even appears from the nice investigations of Messrs. Biot and Arrago on the refractive power of bodies, that the diamond, which has hitherto been considered as pure carbon, must contain a large quantity of hidrogen, which has the greatest refractive power of any substance yet observed in nature. These gentlemen intend to verify their conjecture by direct experiments, from which very interesting results may be expected. The existence of hidrogen in the diamond has been announced from other facts by Mr. Winterl.

General conclusions.

From the chief facts that have been here mentioned it follows:

1. That vegetables find in pure water every thing necessary for them to assimilate.

2. That vegetable mould in a state of complete decay contains nothing soluble, and can only supply plants with water, which it retains abundantly in a certain state of division adapted to their nourishment.

3. That vegetables can grow in any substance, provided it have no action on them, and be perfectly insoluble in water.

4. That the organic powers, assisted by the solar light, developes in plants substances that have been deemed simple, as earths, alkalis, metals, sulphur, phosphorus, charcoal, and perhaps too nitrogen, that probably will no longer continue to be the limits at which chemical analysis will stop.

5. That oxigen, hidrogen, and fire appear to be the only elemen-

elementary substances, that serve to constitute the universe.

6. Lastly that nature, in its simple course, produces the most various effects by the slightest modifications in the means it employs.

IV.

On Vegetable Mucilages; by John Bostock, M. D. *of Liverpool.*

Vegetable mucilage a vague term.

THE term mucilage is employed, in rather a vague manner, to designate a class of vegetable productions, which, although they agree in many of their properties, are in other respects considerably dissimilar.

The author's object.

My object in the following course of experiments was to obtain a more accurate knowledge of their peculiar properties, and to discover tests by which their presence may be detected, without having recourse to those methods of analysis, in which they are resolved into their component elements.

Solution of gum arabic.

Gum arabic, when dissolved in water, exhibits all the properties of a vegetable mucilage in the most complete form. I prepared a solution, in the proportion of ten parts of water to one of gum, and to portions of this the following reagents were respectively added; in general one drachm of the solution was mixed with ten drops of the reagent, except in the case of alcohol and the infusion of galls, when equal parts were employed; 1. Acetate of lead, 2. superacetate of lead*, 3. nitro-muriate of tin, 4. nitro-muriate of gold, 5. nitrate of mercury, 6. oxysulphate of iron, 7. silicated potash, 8. alcohol, and 9. infusion of galls.

Examined with different reagents.

Their effects.

In No. 1, there was a copious, dense, white precipitate. No effect was produced in Nos. 2, 3, and 4. In No. 5 a white precipitate appeared, which was dissolved by agitation, but was reproduced by the addition of water, and in a few hours it assumed a light pink colour. In No. 6 an orange coloured precipitate was formed, at first in small quantity, but in 24

* For the difference between these two salts see Nicholson's Journal, XI, 73; and Thomson's Chemistry, III, 262. (3d Edit.)

hours

hours the whole became opake. In No. 7 an immediate opacity was produced, and after some time a precipitate fell down. There was an immediate precipitate in No. 8: but in No. 9 there was no effect produced. If the oxysulphate of iron be added to a solution containing $\frac{1}{5}$ of its weight of gum, the whole is immediately converted into a solid, transparent, orange coloured jelly. When the solution is so far diluted as to contain only a thousandth of its weight of gum, alcohol no longer produces any visible effect; while a strong solution is immediately converted into a white, and perfectly opake fluid.

Cherry tree gum affected differently by reagents.

A substance which, in its physical properties, bears a strong resemblance to gum arabic, is the gum which exudes from the cherry tree; but I found the effects of reagents upon it to be considerably different. When the acetate of lead is added to a mucilage of cherry gum, there is no precipitate thrown down, but there appears a slight tendency to coagulation, and in the space of 24 hours the gum appears to be separated from its solvent in the form of fine filaments. The nitro-muriate of tin converts the mucilage into a solid jelly of a light yellow colour; the oxysulphate of iron causes no precipitation or coagulation, but changes the colour to a blackish brown; the nitro-muriate of gold causes an immediate opacity, and changes its colour to a light brown, but there is no precipitate thrown down; the superacetate of lead and the nitrate of mercury produce no effect.

Part of its solution incorporates with alcohol.

When alcohol is added to a strong solution of cherry gum, a number of filaments are formed, but the greatest part of the mucilage seems to incorporate with the alcohol without undergoing any alteration; the solid gum is not, however, in the slightest degree soluble in boiling alcohol. The infusion of galls produces no effect upon the mucilage of cherry gum.

Separates from water by standing.

The cherry gum, when first dissolved in water, forms a uniform and transparent solution, but after being kept for some days in a warm atmosphere, it gradually exhibits a tendency to separation, a number of dark films are formed in it, which rise to the surface, and the whole becomes slightly turbid *.

* The result of my experiments on cherry gum will be found to differ very considerably from those of Dr. Thomson. *Chem.* V, 48.

Tragacanth

Tragacanth.

Tragacanth is, in many of its physical properties, considerably different from gum arabic, and its habitudes with the chemical reagents appear to be no less dissimilar. Water dissolves this substance with so much difficulty, that it has been said to be absolutely insoluble†. When tragacanth is digested in water, it absorbs a large quantity of the fluid, and is greatly increased in bulk, but even after being kept for some time at the boiling temperature, no proper solution seems to be produced. If, however, the tragacanth in this softened state be strongly rubbed in a mortar with an additional quantity of fluid, a real combination appears to be effected; a mucilage is formed, which possesses a homogeneous consistence, and retains the same state for several weeks, without manifesting any tendency to subsidence. A mucilage of this kind was made with 1 part of tragacanth to 100 parts of water; it was of about the same consistence with that composed of 1 part of gum arabic to 10 parts of water. To this mucilage the same 9 reagents were added as in the former experiments. In No. 1 a copious dense precipitate was instantly produced. In No. 2 there was a slight degree of coagulation, and a precipitate, which was increased in the space of 24 hours. In No. 3 a firm coagulum was instantly formed. No effect was produced in No. 4, except the mucilage was extremely dense, when its colour was changed to a dusky gray, and afterward to a blackish purple; but without any precipitation or coagulation. In the same manner the oxysulphate of iron produced no effect, except the mucilage was of a very strong consistence, when its colour was changed to a deep brown; but there was no precipitate or coagulum. The nitrate of mercury threw down a slight precipitate of a reddish tinge. Silicated potash produced no effect, except a very strong solution was employed, when there was a degree of opacity produced; but the same was observable upon the addition of caustic potash. It was difficult to ascertain the effect of alcohol upon tragacanth, because, however carefully the mucilage was prepared, any farther addition of fluid would not incorporate with it, but produced an appearance of precipitation.

When softened by boiling in water, may be dissolved by trituration.

1 part to 100: equal to 10 of gum arabic.

Effects of reagents on the solution.

A greater portion of fluid would not in

† Duncan's Dispensatory, p. 183. Thomson's Chemistry, V. 46.

In

In this case, however, I conceive that a proper precipitate was formed, because a greater degree of opacity was perceptible, upon the addition of alcohol to the mucilage, than from an equal quantity of water; and after remaining for some time, the effect was evidently increased, the solid matter being separated in a flocculent form. A turbidness was produced upon adding the infusion of galls to the mucilage of tragacanth, but the same difficulty occurred in determining whether there was any specific effect produced by the presence of the tan. I did not find tragacanth to be precipitated by the sulphate of copper, as stated by Dr. Duncan*. corporate with it.

A well known vegetable mucilage is extracted from linseed. By adding a quantity of the seeds to 10 times their weight of water, a fluid was procured of about the consistence of the abumen ovi; when poured from one vessel to another it showed the same tenacity with the mucilage of gum arabic, and it also resembled gum in being indefinitely soluble in water, and in immediately incorporating itself with any additional quantity of fluid. Its chemical properties are, however, considerably different from those of gum. Upon the addition of the acetate of lead a copious, dense precipitate was immediately thrown down; with the super-acetate of lead, and the nitro-muriate of tin, there was a considerable opacity; with the nitrate of mercury a slight precipitate only was formed; while the nitro-muriate of gold, the oxysulphate of iron, and silicated potash, produced no effect. When equal parts of the mucilage and alcohol were mixed, the fluid became slightly turbid, a degree of coagulation was produced, and at length the solid matter was separated in a flocculent form. Linseed mucilage is not precipitated by tan. Mucilage of linseed. incorporates with water like gum; but differs in its chemical properties. Effects of the reagents.

A substance, which in its physical properties, and in the manner in which it is procured, bears a strong analogy to the linseed mucilage, is that derived from the seeds of the quince. A quantity of these seeds, boiled for a few minutes in 40 times their weight of water, produced a fluid of about the same consistence with the linseed mucilage em- Mucilage of quinceseed.

* Ed. Dispensatory, p. 183.

ployed

by boiling flour in water, were somewhat different from those upon starch. The acetate of lead and the nitro-muriate of tin produced, as in the former case, very copious precipitates; the nitrate of mercury caused the fluid to assume a pink colour, and the nitro-muriate of gold a dusky gray, but without any precipitate; the super-acetate of lead, the oxysulphate of iron, the silicated potash, and the infusion of galls had no visible effect. It is necessary to observe, that in the experiments upon starch and paste, the substances themselves being opake, it is difficult to ascertain the effect of reagents upon them, unless it be considerable and immediate.

The effects of reagents not easily observable.

As starch and paste differ from each other principally in consequence of a quantity of gluten which exists in the latter, I thought it necessary to obtain this substance in a separate state, in order that its properties might be examined with more accuracy. I accordingly procured it in the usual manner, and digesting a quantity of it for some days in water, it exhibited marks of partial solution. The fluid was then filtered, and appeared homogeneous, although somewhat opake; in this state it was submitted to the reagents. Precipitates were thrown down by the acetate of lead, super-acetate of lead, and the nitro-muriate of tin. With the nitrate of mercury a precipitate was produced in moderate quantity, which very quickly subsided, while the fluid assumed a beautiful pink hue. With the oxysulphate of iron, and the nitro-muriate of gold, precipitates were also thrown down, the latter of a light brown colour. No effect was produced by silicated potash; but a very copious precipitate ensued upon the addition of the infusion of galls.

Gluten.

Partially dissolved.

Effects of the reagents.

I next wished to ascertain the properties of vegetable jelly, and for this purpose I procured a quantity of it from the pulp of the gooseberry. I could not, however, succeed in freeing it from the acid which it contains, and was, on this account, prevented from observing the operation of the metallic salts and the silicated potash. I found that a precipitate was formed by adding the infusion of gall to it.

Vegetable jelly. From the gooseberry. Not freed from acid.

Precipitated by gall.

I next submitted a strong solution of sugar to the action of the different reagents, but I found that no effect was produced in any of them, except the acetate of lead, which after

Sugar.

some

some time became opake; but as this effect is produced merely by adding it to water, when exposed to the atmosphere, I was inclined to suppose, that the effect was produced independently of the sugar.

General conclusions.

These being all the substances to which I had an opportunity of extending my experiments, I must now inquire whether any general conclusions can be deduced from them. And first, as to the value of the different reagents employed as tests. The acetate of lead is by far the most delicate and copious in its effects, but it can be of little value in discriminating the different species from each other, because it produces its operation almost equally on all of them. With respect to the super-acetate of lead, we may in the first place remark how materially it differs from the acetate, with which, until lately, it was confounded. The acetate of lead was affected by all the substances to which it was applied, except sugar; while the super-acetate* produced no change upon gum arabic and starch, nor had it any specific effect upon quince mucilage; it was affected only in a slight degree by tragacanth, while with the linseed and hyacinth mucilage, and with gluten, tolerably copious precipitates were thrown down. The nitro-muriate of tin does not act upon gum arabic, but is more or less affected by every other kind of mucilage, particularly by those of cherry gum, tragacanth, and quince, and by starch of gluten. We have seen in how peculiar a manner the oxysulphate of iron acts upon gum arabic; it produces a brown colour in a strong solution of tragacanth; and it forms a precipitate with the hyacinth and the gluten; the quince is, as usual, coagulated; while no change is effected on the linseed or the starch. The nitro-muriate of gold is precipitated by hyacinth mucilage and by gluten; the quince, as in other cases, is coagulated by it, but its most remarkable effect is upon the tragacanth, the colour of which it converts to a deep blackish purple.

Acetate of lead the most active test, but not discriminative.

The super-acetate acted very differently.

Nitro-muriate of tin.

Oxysulphate of iron.

Nitro muriate of gold.

The nitrate of mercury throws down a precipitate of a singular nature from gum arabic, and tinges the fluid of a pink colour; the same shade is produced in the tragacanth, the

Nitrate of mercury.

* Dr Thomson must, no doubt, have employed this salt, when he states, that the acetate of lead does not precipitate gum.

hyacinth mucilage, and in the flour paste, and still more remarkably in the gluten; there is a slight precipitate in the linseed, and a coagulation in the quince, but no change of colour. The silicated potash acts only upon gum arabic, and points out its presence when it exists only in a very minute quantity. Tan does not act upon any of the mucilages, except in a slight degree upon tragacanth; it is copiously precipitated by gluten, and also by vegetable jelly. The effects of alcohol have been fully stated. Gum arabic it precipitates from the water in such a manner as to render the fluid completely opake; whereas in the linseed, quince, and tragacanth mucilages, the solid matter was separated in a fibrous form; with the hyacinth mucilage both the pulverulent and fibrous kinds of precipitate were produced; while the cherry gum was only slightly affected by it. Alcohol precipitated starch in the same manner that it did gum arabic.

Silicated potash.

Tan.

Alcohol.

Nitric acid.

The action of the nitric acid on the different mucilages was so similar, as not to exhibit any phenomena, which can assist us in distinguishing them from each other. According to the circumstances of the process, either the saclactic acid, or a mixture of this acid and the oxalic was produced, attended with the usual disengagement of gas. It did not appear, that any use can be made of the sulphuric acid as a test of the different mucilages; I put in practice the process mentioned by Hermbstaedt, for separating gum from mucilage, but in no instance did I perceive the coagulation which he describes. The acid, in a concentrated state, slowly dissolves the different mucilages, and forms with them a thick, black fluid. I did not observe any effect to be produced by the addition of the neutral salts, except that many of them coagulated the quince mucilage. The pure alkalis generally rendered the mucilages more fluid, but they did not exhibit any specific or discriminating effect.

Sulphuric acid.

Neutral salts.

Alkalis.

Arrangement of the vegetable mucilages.

Before we attempt to make any arrangements of the vegetable mucilages, it is necessary to inquire, whether the different varieties are to be considered as all of them homogeneous, or whether at least some of them ought not to be regarded as compounds of two or more of the primary mucilages. Although I am not acquainted with any method, by which the constituent parts of the compounds can be separated

Some of them compounds.

rated from each other, yet I am inclined to believe, that this is the case; and we may conjecture, with some degree of plausibility, that those species are the most simple, that are acted upon by the fewest reagents. In the first place I consider gum arabic to be a homogeneous substance; and we may properly assign to it the specific name of *gum*, to which it is entitled by long usage and general consent. Its precipitation by silicated potash, and the orange coloured jelly which it forms with the oxysulphate of iron, are sufficient to constitute its essential characters; to which may be added the milky precipitate which is formed by the addition of alcohol to it, and the negative circumstance of its not being affected by the nitro-muriate of tin. I am disposed to regard as pure *vegetable mucus* the substance procured from linseed. It is sufficiently characterized by the effect of the superacetate of lead and the nitro-muriate of tin, and by the manner in which it is precipitated by alcohol from its aqueous solution; these circumstances, as well as the negative operation of the oxysulphate of iron and the silicated potash, serve to mark an obvious distinction between this substance and gum. A third vegetable principle, which is possessed of pecular physical properties, is *starch*. In its relations to the different chemical reagents it strongly resembles mucus, although it differs from it in the manner in which it is precipitated by alcohol. *Gluten* is a fourth substance, in every respect essentially different from any which we have hitherto examined. The most remarkable effect produced upon it, by any of the chemical reagents, is the change of colour induced by the nitrate of mercury, and the copious precipitate by the addition of tan.

Gum arabic homogeneous, and gum by way of eminence.

Its characters.

Vegetable mucus from linseed.

Starch.

Gluten.

In endeavouring to form an arrangement of vegetable mucilages, and to assign definite characters for the primary substances, which enter into the composition, we must consider to what degree of minuteness our subdivisions ought to be extended. If, for instance, we meet with a body, agreeing in every physical and chemical property with gum arabic, except that it is not precipitated by silicated potash, are we to regard this as a distinct vegetable principle, or simply as a variety of gum? This latter opinion I should certainly be inclined to adopt; for by indefinitely multiplying our

Extent of subdivision.

our principles, we should defeat the very end of arrangement. Hence, in the formation of our essential characters, we must endeavour to adopt the due medium between the extremes of minuteness and laxity. In each particular instance we must be guided by those properties, which are the most distinctly recognized, and the most readily ascertained, and which exhibit the closest analogy to each other. Under this impression I shall not think it desirable, in the present state of our knowledge, to proceed any farther in proposing an additional number of primary vegetable compounds; but I shall offer a few remarks upon the different mucilages, that have been made the subject of experiment.

Generic characters of gum.

Gum I consider as a generic term, which may be defined a transparent, brittle, insipid substance, indefinitely soluble in water, with which it forms a mucilage; the mucilage is precipitated by alcohol, in such a manner as to render the fluid perfectly opake; it is also precipitated by the acetate of lead in dense flakes. Under this genus we can, at present, rank only one species, viz. gum arabic, to which the following specific character may be applied. A gum, the mucilage of which may be precipitated by silicated potash; forms with the oxysulphate of iron a solid jelly, with the nitrate of mercury a precipitate of a pink colour; and is not acted upon by the superacetate of lead, the nitro-muriate of tin, or the nitro-muriate of gold.

Species and characters.

Generic characters of mucus.

The second genus is mucus, a substance seldom found in a separate state, but forming a frequent constituent of the seeds, roots, leaves, and other parts of vegetables. It is indefinitely soluble in water, and forms with it a mucilage; this is precipitated by alcohol in a fibrous form, without rendering the fluid opake; it is also precipitated by the acetate of lead, the super-acetate of lead, and the nitro-muriate of tin. Under the genus of mucus we may enumerate three species, that of linseed, of quinceseed, and of the hyacinth. To the first of these the generic character strictly applies; and to the second we may add the specific character of being coagulated by the addition of any acid, neutral, earthy, or metallic salt. The hyacinth mucilage I have also classed as a mucus, because its leading properties are such as point out its relation to the substances of this genus. In the state, however,

Three species.

Specific characters.

That of the hyacinth a compound.

however, in which it is usually procured, it seems to be a compound of two or more of the vegetable principles. A quantity of starch is obviously mixed with it; and I am inclined to think, that it also contains gluten, a supposition which will account for all the phenomena it exhibits with the chemical reagents.

Cherry gum.

There are two substances, the characters of which still remain to be ascertained, cherry gum and tragacanth. From its physical properties we should be disposed to place cherry gum in the same class with gum arabic; but it is so differently affected by the chemical reagents, as absolutely to prevent us from considering it in this point of view. Its properties are not more characteristic of mucus; nor does it afford any indications of being a compound substance, so that it may probably be necessary to consider it as a distinct vegetable principle. The action of the acetate of lead and of alcohol upon cherry gum would induce us to suppose, that it bore an analogy to sugar, rather than to the gums or mucuses; but the effect of the nitro-muriate of tin is not favourable to this supposition; nor is that of the nitric acid, which I found, by the usual process, converted it principally into the saclactic acid. If we are to bestow a new name upon it, we might denominate it cerasin.

Perhaps a distinct principle

Cerasin.

Tragacanth.

I am equally unable to determine in what class tragacanth ought to be placed. It has obviously no relation to gum, either in its physical or chemical properties; and it differs very considerably from what has been laid down as the generic character of mucus. Its properties are the most similar to those of starch and gluten; particularly to that form of starch, which is prepared by first forming it into a mucilage with hot water, and then evaporating it to dryness, when it becomes transparent and brittle, but almost insoluble in water. The effect of the nitro-muriate of gold forms a remarkable character of tragacanth mucilage; it seems to depend upon a partial reduction of the oxide, at the same time that it unites with the tragacanth. The effect was very evident when the solid matter composed 1-50 part of the fluid, but if much more diluted, it was not perceptible; a slight degree of the same effect is produced when the nitro-muriate of gold is added to flour paste.

I am

I am fully sensible, that I have by no means executed the task, which I proposed to myself, of characterizing and arranging the vegetable mucilages; but a particular circumstance having, for the present, put a stop to my experiments, I was induced to publish this very imperfect attempt, in hopes, that the difficulties being pointed out, some more able hand may endeavour to remove them.

Liverpool, Aug. 12, 1807.

V.

Observations on Sulphurous Mineral Waters; by Mr. WESTRUMB*.

Sulphurous waters contain hidrosulphuret of lime.

MR. Westrumb has examined various sulphurous waters, and lately those of Eilsen in the county of Schaumbourg. One of the most interesting facts he has observed is, that all sulphurous waters contain more or less hidrosulphuret of lime.

The gasses being expelled by boiling, this is shown by sulphuric, nitric, or oxalic acid.

To detect this he boiled the mineral water, excluding the contact of atmospheric air, to expel the sulphuretted hidrogen gas and carbonic acid. Into the water thus boiled he poured sulphuric acid, when more sulphuretted hidrogen gas was evolved, and sulphate of lime was thrown down: fuming nitric acid, which separated from it sulphur: and oxalic acid, which expelled sulphuretted hidrogen, and formed oxalate of lime. The water evaporated in open vessels let fall sulphate of lime, and gave out sulphuretted hidrogen gas.

Method of ascertaining the quantity of sulphuretted hidrogen and carbonic acid gas in them.

To ascertain the quantity of sulphuretted hidrogen gas and carbonic acid, Mr. Westrumb proceeded as follows. He introduced the sulphurous water into a matrass, till it was filled to a certain point, which he marked; fitted to it a curved tube, which terminated in a long cylinder; filled this cylinder with limewater for the one experiment, and with acetate of lead with excess of acid for the other; luted the apparatus;

* Originally published in Gehlen's new Journal of Chemistry, and abridged by Vogel, Annales de Chimie, vol lxii, p. 183, May, 1807.

the

and boiled the water till no more gas was expelled. When the lime-water is used carbonate of lime is precipitated in the proportion of 20 grains to every 10 cubic inches of carbonic acid gas; when the solution of acetate, hidrosulphuret of lead is thrown down in the proportion of 19 grains to 10 cubic inches of sulphuretted hidrogen gas.

Sulphuretted nitrogen gas

Another observation, not less remarkable, relates to sulphuretted nitrogen gas.

in the waters of Aix-la-chapelle and Nenndorf.

It is known that Dr. Gimbernat, a Spanish chemist, asserts, that the thermal waters of Aix-la-Chapelle contain sulphuretted nitrogen gas. Mr. Schaub too says, that he has obtained it from the sulphurous waters of Nenndorf in Hesse.

Its characters.

The following characters are ascribed to this gas. 1. In smell it resembles sulphuretted hidrogen. 2. It is not decomposable by carbonic acid. 3. It is not inflammable. 4. It will not maintain combustion. 5. It is not decomposable by nitrous acid. 6. It is not decomposable by concentrated nitric acid, which separates from it sulphur. 7. It decomposes metallic solutions, and forms sulphurets. 8. It has a great affinity for water, from which it is separable only by long boiling.

Sulphuretted hidrogen gas has the same properties imparted to it by lime.

But Mr. Westrumb has found, that sulphuretted hidrogen gas, when washed with milk of lime, or passed through lime diluted with water, acquires all the properties here mentioned. Whether the sulphuretted hidrogen gas be obtained from sulphurous waters, or prepared artifically, the same phenomena take place. If the milk of lime be taken from it by an acid, sulphuretted hidrogen is disengaged, which is inflammable, and possesses the usual properties. Sulphuretted nitrogen gas therefore is a product of the operation.

Whether a product or an educt doubted.

Mr. Westrumb however is in doubt, whether this new gas be produced by the action of quicklime on sulphuretted hidrogen, or whether the sulphuretted hidrogen gas contain sulphuretted nitrogen.

Carbon in sulphurous waters.

A third observation, not less interesting, is the presence of carbon and carburetted substances in sulphurous mineral waters.

A fetid resin of sulphur found in them.

Mr. Westrumb has discovered in them a new principle, a fetid resin of sulphur *(stinkendes schwefelharz)*. To obtain this, the sulphurous water must be evaporated in open vessels

vessels, and the residuum dissolved in alcohol, which takes up this resin and the earthy muriates. By evaporating the alcohol, this substance appears at first as a yellowish fat, which gradually assumes a brown colour, and becomes resinous. By repeated solutions in alcohol and evaporations it is decomposed into sulphur and a blackish brown resin. It emits a garlic smell; which becomes very strong, and similar to that of assafetida, if water be poured into the alcoholic solution. Its solution acts as an acid.

Smells like garlic.

Soluble in ammonia.

Forms hidrosulphurets.

The resin is soluble in ammonia, and communicates to it a yellow colour. This liquid comports itself like that of Beguin. With limewater a hidrosulphuret is formed. All these solutions act on metallic compounds in the same manner as sulphuretted hidrogen.

Perhaps from the bitumen of coal.

As sulphurous mineral waters arise from strata of pitcoal, perhaps the source of this bituminous principle may be traced to the coal itself.

Black mud of the baths of Eilsen.

Round the baths of Eilsen, as round those of St. Amand, a mud accumulates, which in time grows darker coloured, and ultimately black. From this are obtained, on analyzing it, fetid sulphurous resin, hidrosulphuret of lime, sulphur, lime, alumine, magnesia, charcoal, and sand, with some fibrous substances; and a little sulphuretted hidrogen gas, and carbonic acid gas.

Charcoal and the fetid resin produced from sulphur.

Whatever may be the origin of the bituminous principle in the sulphurous waters, Mr. Westrumb, assisted by Mr. Basse, has been able to produce charcoal and the fetid resin from pure sulphur. For this purpose he has digested in alcohol sulphur precipitated from sulphuretted hidrogen by an acid. On distilling off part of the alcohol, sulphur is separated in yellow crystals, or in a yellowish gray powder. The fetid resin is then completely formed in the supernatant liquor, and possesses all the properties mentioned above.

Not formed by the alcohol.

Its formation may be ascribed to the concurrence of the alcohol; particularly as after its separation from the residuum left on evaporating sulphurous water, the pungent smell is manifested on its being taken up by alcohol. But several observations lead Mr. Westrumb to believe, that alcohol does not contribute to the formation of this substance.

stance, but rather that it derives its origin from the sulphur itself.

Messrs. Westrumb and Basse intend to pursue the inquiry, and promise to make known the results. The inquiry will be pursued.

VI.

Report on a Memoir of Mr. BERTHOLLET, *Jun., entitled: Inquiries concerning the reciprocal Action of Sulphur and Charcoal; by Messrs.* FOURCROY, DEYEUX, *and* VAUQUELIN *.

IN 1796 prof. Lampadius, of Freyberg, endeavouring to ascertain how much sulphur a martial pyrites mixed with charcoal would furnish when acted upon by fire, obtained a very volatile sulphurous liquid, which he suspected to be a compound of sulphur and hidrogen, and to which he afterward gave the name of sulphur alcohol. Lampadius's sulphur alcohol, hidroguretted sulphur.

Clement and Desormes, unacquainted with this fact, when afterward endeavouring to demonstrate, that there is no hidrogen in well burned charcoal, by passing sulphur in vapour over very hot charcoal, obtained a product exactly similar to that of Lampadius. Having remarked, that the charcoal in this operation was destroyed without evolving any gas; and that the liquid product, when burned, left some slight black spots on the vessel that contained it; they thought, that this liquor was formed by the combination of sulphur with charcoal, and consequently termed it carburetted sulphur. Carburetted sulphur of Clement and Desormes.

This difference of opinion respecting its composition induced Mr. A. B. Berthollet, to examine experimentally this question, with which many interesting points in chemistry are connected. Examined by Berthollet jun.

His apparatus, which does not differ much from that of Clement and Desormes, was constructed in the following manner. A straight glass tube, about a yard long, was Description of his apparatus.

* Annales de Chemie, Vol. LXI. p. 127. Feb. 1807.

placed

placed in a reverberatory furnace, so that one end projected a little more than four inches beyond the wall of the furnace; and the other end, which sloped gently upward, about eighteen inches. The part within the furnace was coated with a lute capable of sustaining a very strong heat. To the lower end was fitted an adopter, terminating in a small tubulated receiver, from which a curved tube passed into water contained in a two-necked bottle. Another tube with two bendings conveyed the gas from this bottle into a pneumatic apparatus.

Charcoal included in the tube, with sulphur above it, and heat applied.

Charcoal being included in that part of the tube that was luted, and sulphur introduced into the empty part, its upper end was hermetically sealed; the tube was gradually heated to incandescence; and the sulphur as it melted flowed down into the charcoal. When these came into contact,

Products.

bubbles of gas were extricated, that succeeded each other rapidly, and were accompanied with white vapours, which, condensing in the adopter, passed into the receiver, and sunk to the bottom of the water in the form of a white or sometimes yellowish oil.

These liable to vary.

Mr. Berthollet however observes, that the results of this operation vary according to a number of circumstances, which he has carefully described; and the chemist not having it always in his power to render these circumstances perfectly similar, different products are frequently obtained.

Circumstances that affect them.

If for instance the extrication of gas and condensation of liquid slacken, the sulphur must be heated, that more may pass down; and if this do not accelerate the operation, the temperature of the charcoal must be increased. When the operator is desirous of producing much of the liquid, it is necessary to raise the temperature of the charcoal a little above a cherry red, and to allow only a slight excess of sulphur to pass down. Too little of the sulphur produces only gasses, and a few drops of a liquid lighter than water, which in the course of the process resumes the state of gas. On the contrary, if the sulphur be too abundant, nothing is formed but gasses, and solid hidroguretted sulphur, which was mistaken for carburetted sulphur by Clement and Desormes. It is always advantageous, to keep the vessels, in

The receivers

which

which the liquid is to be condensed, in a refrigerating mixture. to be kept very cool.

In conducting the operation with these precautions, the extrication of gas will not take place after a certain time, unless both the temperature of the tube and the quantity of sulphur be augmented. Two stages in the process.

The charcoal used by Mr. Berthollet was always previously heated for half an hour, to expel the water and gasses that yield to simple heat. When the operation was finished, he kept up the temperature of the tube, that the nature of the residuum might not be altered by the sulphur in contact with it; and for the same reason he prevented all access of air to the apparatus, by turning a cock adapted to the tubulure of the bottle. Necessary precautions.

On examining the products of the operation, stopped at the end of the first stage, he found: 1. That the water in the bottle, which was milky, had the smell and all the properties of sulphuretted hidrogen water. 2. That the gas itself had a similar smell, dissolved in water by agitation or long contact, and communicated to it all the characters of sulphuretted hidrogen. 3. That this gas burned with a blue flame, and diffusing a smell of sulphurous acid. 4. That when mixed with oxigen gas it detonated briskly with the electric spark, sometimes without rendering limewater turbid, but more frequently producing a slight precipitate, and depositing sulphur. Products of the first stage.

From these characters every one must recognise in it sulphuretted hidrogen; though Mr. Berthollet found, that it was less soluble in water than what is obtained by the ordinary means. Scheele, Kirwan, and others, however, have mentioned combinations of sulphur and hidrogen, which were little, if at all, soluble in water. Evidently sulphuretted hidrogen, though little soluble in water.

The liquid collected under the water in the receiver, and in the bottle, had perfectly similar properties to those described by Lampadius, and by Clement and Desormes: that is, it was as transparent as water; it emitted a smell resembling that of sulphuretted hidrogen, but more lively and pungent; shaken in a phial with water, it adhered to the glass, and rendered it greasy like an oil; and it burned rapidly, with a blue flame, and smell of sulphurous acid. A liquid like that of Lampadius.

He

Left no charcoal when burned.

He did not obtain any charcoal, however, as a residuum of its combustion; the circumstance on which Clement and Desormes founded their opinion: for it burned entirely away, or, if the combustion were stopped before it was completed, left nothing but sulphur.

Its characters.

This liquid, being very volatile, produces on the skin a sensation of great cold. It dissolves in the air, greatly increasing its volume; and then burns calmly with a blue flame, and does not detonate by the electric spark. If water be admitted to the air thus expanded, the air returns to its original bulk, and the water acquires the properties of sulphuretted hidrogen. This Mr. Berthollet observes would of itself be sufficient to prove the existence of hidrogen in the liquid.

Evidently contains hidrogen.

Not entirely volatile, sulphur being left.

However transparent it was, he could never volatilise it entirely. Whether he left it exposed to the air, or assisted its evaporation by heat, he had always a residuum of sulphur, which he could sublime completely, without perceiving any vestige of charcoal.

The gasses it produce afforded no indications of charcoal.

The residuum of this liquid affording Mr. Berthollet no charcoal, he examined the gasses it produced, to ascertain its existence. But neither its combustion with oxigen gas in vessels placed over water, nor the action of oxigenized muriatic acid, nor that of alkalis, produced any indications of charcoal, or of carbonic acid. In the first case the product of its combustion did not render limewater turbid; in the second nothing was found but sulphuric acid mixed with muriatic; and in the third a combination was obtained, which comported itself like the hidroguretted sulphurets.

It is therefore a compound of sulphur and hidrogen.

From all these facts the author concludes, that the liquid produced by the reciprocal action of incandescent charcoal and sulphur is formed of sulphur and hidrogen, as Lampadius announced; and, contrary to the assertion of Clement and Desormes, contains no charcoal. These facts at the same time show, that sulphur and hidrogen, like many other substances, are capable of uniting in various proportions, according to circumstances; and that the predominating ingredient always communicates some of its properties to the compound. In the present case for instance, if the sulphur

These unite in various proportions,

be

be very abundant, the compound takes the solid form: if the proportion of hidrogen be increased, the attraction of its particles is diminished, and it resolves itself into a liquid: if a still greater quantity of hidrogen be present, the compound expands, and the result is a gas.

which alter the characters of the compound.

Mr. Berthollet has made a very interesting experiment, which greatly confirms this. The liquid in question, distilled with water at a temperature of 36° [113° F.] afforded him a gas, that had the smell of sulphuretted hidrogen, burned with a blue flame, detonated briskly with oxigen when fired, and combined readily with water, which it turned milky, communicating to it the properties of sulphurous water. After this gas a transparent liquor came over, swimming on water, and which, as it evaporated on the contact of air, precipitated to the bottom, or disappeared entirely, leaving only some slight traces of sulphur on the water. At 45° [133°] the extrication of gas ceased, and a liquid heavier than water succeeded. The colour and consistency of this liquid increased, as the distillation proceeded.

The liquid distilled with water at a low heat gave a gas,

a light liquid,

a heavy liquid,

On stopping the process when the temperature had been kept some time at 45° [133°], what remains in the retort becomes solid by cooling, and prismatic crystals are distinguished in the mass. If a sufficient quantity of sulphur be not melted down on the charcoal, liquids of different densities are likewise obtained: the heaviest condense in the receiver; the lighter do not condense, till they reach the bottle, where they rise to the surface of the water; and lastly others, carried off by the gasses, reach the pneumatic apparatus.

and a solid remained.

When the sulphur is deficient liquids of different densities obtained.

It is evident, that the efficient cause of these different modifications, depending on the respective qualities of the elements that combine, is the difference of temperature, which disengages first the most expansible bodies. This is not peculiar to the compound of sulphur and hidrogen; and the effect is the more obvious, the greater the difference in the expansive force of the substances.

These modifications owing to difference of temperature.

The sulphur that flows into the adopter during the operation contains a certain quantity of hidrogen, which gives it a laminated texture, an inferior density, and in particular a very decided smell of sulphuretted hidrogen, a small quantity

The sulphur that flows by the charcoal contains hidrogen,

tity of which Mr. Berthollet obtained from it by means of a gentle heat.

but no charcoal. Some manganese and iron.

But by no method could he discover in it charcoal; though he imagined he discerned some very slight traces of manganese and of iron, arising from the charcoal, or from the sulphur itself.

The same products when hidroguretted sulphurets are decomposed by acids.

A fact long known, that confirms the conclusions of Mr. Berthollet, is, that the same products may be obtained by decomposing hidroguretted sulphurets by means of acids, as by distilling sulphur over charcoal, namely, sulphuretted hidrogen in the state of gas, liquid hidroguretted sulphur, and solid hidroguretted sulphur; and in all these substances there is no charcoal.

The charcoal not altered in appearance, but combined with sulphur.

On examining the charcoal remaining in the apparatus after having been long exposed to the heat, Mr. Berthollet found no external appearance of alteration. It retained sulphur in actual combination, which heat could not separate, but which might be dissolved by an alkali, or burned by heating in contact with air. The charcoal is then very light and friable, leaves fine black traces on paper, and burns with difficulty. Charcoal therefore can combine with sulphur; but this compound assumes neither the liquid nor the gaseous state.

Charcoal contains hidrogen;

All the facts adduced by Mr. Berthollet clearly demonstrate the presence of hidrogen in charcoal, from which it is inseparable by any heat we have yet been able to produce.

which sulphur aided by heat, takes from it.

If sulphur take it from charcoal, it is by combining its chemical action with that of heat: and perhaps this may be a means of depriving charcoal entirely of hidrogen, and obtaining it in a state of purity, so as to describe its properties, which from this observation may be yet unknown to us.

The whole of the charcoal may be made to disappear;

Mr. Berthollet has remarked however, that, when all the phenomena already described have taken place, if the temperature be strongly raised, and a great deal of sulphur caused to pass, the extrication of gasses recommences, and the charcoal may be made to disappear entirely. On stopping the process before this arrives, pieces of charcoal exhibiting evident marks of erosion will be found in the tube. The little liquid obtained in this second stage of the process is so volatile, that it soon reassumes the state of gas. The sulphur that flows into the adopter contains no more charcoal,

coal, than what passed at the commencement, but it contains hidrogen. An experiment made on a gramme [15½ grs.] of charcoal, previously heated for an hour in a forge fire, continued five or six hours, and produced four or five litres [or wine quarts] of gas.

This gas resembled sulphuretted hidrogen in its smell, manner of burning, solubility in water, and the properties it communicated to water. But water did not absorb it completely; and the product of its combustion rendered limewater considerably turbid. In this gas therefore, analogous probably to that which Clement and Desormes termed gaseous carburet of sulphur, the carbon subjected to the experiment is found. It is in fact a triple compound of carbon, hidrogen, and sulphur; requires for its combustion nearly an equal bulk of oxigen gas; and at the moment of taking fire has its volume increased at least tenfold. and a gaseous compound of carbon, hidrogen, and sulphur, is formed.

With respect to the doubts, that may arise respecting the nature of the precipitates formed in limewater in these experiments, Mr. Berthollet gives means of ascertaining, whether they be owing to sulphurous acid or carbonic.

Reflecting on the complete destruction of charcoal by the action of sulphur, and the nature of the products it furnishes, we are tempted to believe, that charcoal is inseparable from a certain quantity of hidrogen; and that, at a high temperature, the sulphur in contact with it occasions a new order of compounds, which assume the elastic state. Some hidrogen essential to charcoal.

But on considering the quantity of gas obtained, and the property sulphur has of retaining hidrogen in the solid state, Mr. Berthollet suspected, that possibly the sulphur itself furnished a certain quantity of the gas. To verify this ingenious suggestion, he passed sulphur through a glass tube, coated, and brought to a white heat; adapted to it a tube, for collecting the gas; and obtained some very slight indications of sulphuretted hidrogen. The sulphur too probably contains some. Experiments to prove this.

On the other hand, on forming metallic sulphurets in earthen retorts, after taking all possible precautions to remove every source of uncertainty, he obtained sufficient sulphuretted hidrogen gas to precipitate the solution of lead, and to be set on fire. In these experiments he employed From the formation of metallic sulphurets.

iron prepared expressly for the purpose, copper, and mercury. The last metal afforded him the most.

Aqueous vapour passed over melted sulphur takes up hidrogen, without being decomposed.

On this occasion too he repeated an experiment of Priestley's, who produced sulphuretted hidrogen gas, by passing the vapour of water over melted sulphur: and he found, that the water was not decomposed, for he could discover no trace of sulphuric acid; it only served therefore, to disengage the sulphuretted hidrogen. Many other facts in confirmation of these experiments might be adduced if necessary.

General conclusions.

From the experiments of Mr. Berthollet we may conclude:

1. That charcoal contains hidrogen, which the most intense heat we can produce will not completely expel.

2. That sulphur at a red heat acts upon hidrogen, and forms compounds in very different proportions, on which their properties depend.

3. That charcoal deprived of hidrogen, or at least nearly so, forms with sulphur a solid compound, into which the sulphur enters in a small proportion.

4. That at a high temperature sulphur, carbon, and hidrogen unite into a compound, which assumes the state of gas.

5. And lastly, that sulphur contains hidrogen.

Experiments by Robiquet confirm those of Berthollet.

While Mr. A. B. Berthollet was examining the nature of this compound, Mr. Robiquet, apothecary to the hospital of Val-de-grace, was likewise making experiments on it, at the suggestion of Mr. Vauquelin, to whom Mr. Berthollet's intentions were unknown. These led to similar conclusions; but were discontinued, as soon as Mr. Vauquelin was acquainted with the labours of Mr. Berthollet. Mr. Vauquelin had likewise given a tolerable quantity of the liquid to Mr. Biot, that he might ascertain, if possible, from its refractive power, the proportion of hidrogen it contains.

Biot supposes sulphur to contain hidrogen.

We may add, that Mr. Biot had already inferred the presence of hidrogen in sulphur, during the course of his experiments on refraction.

VII.

VII.

Account of the Metallurgic Treatment of Pyritous Copper at the Mines of Chessy and Sainbel, in the Department of the Rhone: by Mr. GUENIVEAU*.

THIS paper contains several results of the analysis of the metallurgic products of the works at Chessy, which appear calculated to serve as bases for the theory of smelting copper pyrites, and particularly to show the effect of quartz added to the ore in this operation. The fifth part is employed in describing those chemical operations, that afforded new results; and the remainder in deriving from those results the most remarkable consequences, and pointing out their application.

Products of the works analysed to explain the theory of smelting copper pyrites, and the effect of quartz;

The chemical experiments were made in part at the laboratory of the School of Mining of Mont Blanc, under the inspection of Mr. Hassenfratz, and partly at the laboratory of the Council of Mines, under Mr. Descotils.

in concert with Hassenfratz and Descotils.

The metallurgic products subjected to analysis were scoriæ from the pyritous copper, taken from the basin of the fore hearth during the operation. Of these there are two sorts, one formed during the fusion of the roasted ore, the other during that of the roasted matts. The first sort is of a tolerably brilliant metallic gray colour; exhibits laminæ, indicating a crystallization; and may easily be confounded with certain ores of oxidulous iron. The second has no metallic lustre, is of a brown colour, and of a fibrous fracture. The characters common to both are, they are tolerably compact, without any vitreous appearance, and almost without blebs; attractable by the magnet; melt before the blowpipe without addition, sometimes emitting a slight smell of sulphur, and with borax exhibiting the same characters as iron ores; yielding iron when assayed in the dry way, and sometimes traces of copper; and forming a jelly with acids with great facility.

They were the scoriæ of the ore, and of the matts.

Characters of the 1st,

of the 2d,

common to both.

The following is a general description of the mode in which they were analysed. After they were powdered, they

General mode of analysis.

* Abridged from the Journal des Mines, Oct. 1806, Vol. xx, p. 345.

were treated with concentrated muriatic acid, mixed with a little nitric acid. With this they formed a tolerably tenacious jelly, which was diluted by adding water, and boiling it. The insoluble part, become very white, was separated, dried, weighed, and then heated red hot, to expel the sulphur. The residuum had all the characters of silex. The muriatic solution contained no sulphuric acid. To this ammonia was added in excess, which gave indications of copper and dissolved the zinc. The precipitated oxide of iron was treated while wet with a solution of caustic potash, to separate the alumine. Into the ammoniacal liquor oxalate of ammonia was poured, to separate the lime: after which an excess of sulphuric acid was added, in order to precipitate the copper by a slip of iron. By a large quantity of ammonia the zinc was separated from the iron introduced; and the proportion of this metal was determined by converting it into a prussiate.

The first precipitate of oxide of iron was examined anew, in the hope of finding in it portions of copper and of lime, which might have escaped the first operations. The sulphur, copper, and zinc, were examined by separate experiments.

Component parts of the scoriæ No. 1.

From 100 parts of the scoriæ of the roasted ore were obtained silex 31, red oxide of iron 75, metallic zinc 2. Beside which there were some indications of copper and sulphur, and an atom of lime.

No. 2.

From 100 parts of the scoriæ of the roasted matt were obtained silex 22, red oxide of iron 90, lime 3, sulphur 3; beside a trace of copper and of zinc.

Assay gave 50 of iron.

The assay in the dry way, with Guyton's flux, gave 50 per cent of iron.

The iron slightly oxided.

The iron in these scoriæ is very little oxided, for the weight of all the products of the analysis exceeds that of the substance employed, and ammonia precipitates the muriatic solutions of a green colour*. It is observable too, that the second kind of scoriæ contained much more iron than the first.

Some

State of the iron doubtful.

* The state of the iron in this combination appears to me very doubtful: for, if we consider it as black oxide, that is with 27 per cent of oxigen, it is not easy to explain the effervescence, that the powdered scoriæ

Some time afterward Mr. Gueniveau examined a specimen of scoriæ of the first kind, obtained in like manner from the fusion of the ore, which was in the collection of the Council of Mines. His principal objects were to ascertain whether it contained manganese, and whether the insoluble residuum had a mixture of sulphate of barytes: but as the component parts of this specimen differed from those of the former, he gives the following account of his analysis. Another specimen, No. 3, analysed for manganese and sulphate of barytes.

He treated 5 gram. [77grs.] of the scoriæ, thoroughly pulverised, with ten of caustic potash, in a silver crucible. The fusion was complete; and on its being suffered to grow cold, and a large quantity of water being added, no green colour appeared, which evinced, that no manganese was present. Analysis. No manganese.

Another portion he treated with concentrated muriatic acid mixed with a little nitric, to detect the alkalis. Having separated the insoluble residuum, he treated the solution with ammonia, which separated a part of the earths and metals. The copper and zinc dissolved were then precipitated by sulphuretted hidrogen; the excess of this gas was expelled by ebullition; the lime was thrown down by oxalate of ammonia; and the liquor was evaporated to dryness. The ammoniacal salts were then driven off by bringing the residuum to a red heat in a platina crucible, and a pretty considerable quantity of fixed salts remained. Having dissolved these in a very small quantity of water, he added to the solution muriate of platina, which occasioned no precipitate; and endeavoured, but in vain, to obtain crystals by cautious evaporation and cooling; which convinced him, that neither potash nor soda was present. Examined for alkalis. Neither potash nor soda.

By various trials he found, that the fixed residuum consisted of the muriates of barytes and magnesia. Barytes and magnesia.

Lead too was an object of his particular research; but he could not discover an atom. No lead.

scoriæ make with sulphuric acid a little concentrated, and with dilute muriatic acid; an effervescence owing to the decomposition of water, since the gas has the smell of hidrogen set free by iron. The same phenomenon has been observed in scoriæ from sparry iron ores, which were not attractable by the magnet, and contained a great deal of manganese, and but little iron.

From

From these data Mr. Gueniveau undertook a complete analysis of this specimen of the scoriæ of Chessy.

Its external characters.

Its external characters were the same as those of the scoriæ No. 1. Its specific gravity was 3·61.

Assay.

Assayed with borax it gave 23 per cent of iron, without any signs of copper.

Analysis.

Five grammes, well powdered, were treated with concentrated muriatic acid, mixed with a little nitric.

Treated with concentrated muriatic acid, and a little nitric.

The whole coagulated into a jelly. On this water was poured, it was boiled, and fresh acid was added; by which means a very white residuum was obtained. The decanted liquors were mixed, and evaporated to dryness, in order to separate the silex completely from the muriatic acid. Water being added, and filtered, a residuum was collected, which contained no sulphur, and weighed 1·75 gr. after having been exposed to a red heat. This was fused with very pure caustic potash; and the whole of the compound being soluble in muriatic acid, Mr. Gueniveau inferred, that the residuum was very pure silex, without any mixture of sulphate of barytes.

Residuum pure silex.

Muriatic solution precipitated with sulphuric acid,

The muriatic solution gave with sulphuric acid a precipitate of 0·90 gr. of sulphate of barytes, with which he ascertained no sulphate of lime was mixed, and which was heated red hot before it was weighed.

and hidrosulphuret of ammonia.

The same solution was treated with hidrosulphuret of ammonia, which precipitated the metals and alumine, leaving in solution lime and magnesia. The sulphurets were roasted, and afterward dissolved by nitro-muriatic acid: and lastly, the sulphur and sulphuretted hidrogen were separated from the preceding liquor. He had then two solutions; one, A, containing lime and magnesia; the other, B, containing the metals and alumine.

Lime thrown down by oxalate of ammonia,

From the solution A the lime was thrown down by oxalate of ammonia; and the precipitate, being strongly calcined, left 0·18 gr. of caustic lime.

and magnesia by potash.

The magnesia, which remained, was precipitated by potash; and, after being calcined, weighed 0·1 gr.

Iron and alumine precipitated by ammonia, and separated by potash.

The solution B was treated by ammonia, an excess of which dissolved the zinc and copper. The oxide of iron and the alumine, which fell down together, were separated by liquid caustic potash, assisted by heat. The red oxide of iron after calcination weighed 2·30 gr.; and the alumine, after

after exposure to a red heat, 0.21 gr. As the ammoniacal solution of copper and zinc had a very slight blue colour, it did not appear necessary, to separate the few atoms of copper: so the whole was precipitated together by carbonate of potash, assisted by a boiling heat. The carbonate, being calcined, gave 0·17 gr. of zinc mixed with a very small quantity of copper. The quantity of copper and zinc was ascertained likewise by separating them from the iron by sulphuretted hidrogen, and it did not differ perceptibly from this.

Copper and zinc by carbonate of potash.

One hundred parts of these scoriæ gave therefore silex 35, barytes, not sulphurated, 12, alumine 4, lime 3, magnesia 2, red oxide of iron 46, oxide of zinc 3, beside some traces of copper and of sulphur.

Component parts of scoria No. 3.

This specimen contained much less iron than No. 1, which was formed under the same circumstances: but the difference will not appear surprising to those, who know how much the proportions of the gangue, and foreign matter added to the ore, vary in different smeltings. The following are the most important consequences deducible from these analyses.

Less iron than No. 1.

1. The predominant parts of the scoriæ of the pyritous copper of Chessy are oxide of iron and silex, the rest being variable, and in small proportion.

General inferences.

2. The combination of the silex with the oxide of iron is effected in the furnaces in which the ore is smelted.

3. The presence of barytes, not sulphurated, announces, that the sulphate of barytes, which made part of the gangue, is decomposed. No doubt it had been converted into a sulphuret, the sulphur of which was taken up by the iron or copper, and the free barytes remained in the scoriæ.

4. The scoriæ arising from the smelting of the copper pyrites and matts may be considered as iron ores, since they yield a considerable quantity of iron when assayed.

The application of chemistry to the art of treating ores, observes Mr. Gueniveau, is considered by all metallurgists as one of the most certain means of elucidating its operations, and consequently improving its processes: and accordingly he relates in a few words the principal parts of the metallurgic treatment of the pyritous copper of Chessy, from

Chemistry the means of explaining metallurgical processes.

from the account given by Messrs. Lemaire and Bouësnel, in order to render more perspicuous the comparison between the chemical and metallurgical results.

Treatment of the ore of Chessy, which is very poor.

The greater part of the ore wrought at Chessy is very poor in copper. It is a mixture of sulphuretted iron, sulphuretted zinc, pyritous copper, and a small portion of earthy substances, as quartz and sulphate of barytes. The ore after pounding is roasted by a very economical process, which saves the greater part of the sulphur contained in it. It is then smelted, with the addition of scoriæ from preceding smeltings, and quartz broken to pieces, the proportion of which varies from seven to ten hundredths of the weight of the ore.

Pounded, roasted, and smelted with the addition of scoriæ.

This smelting affords two products; the scoriæ, of which analyses have been given, Nos. 1 and 3; and matts, or sulphurets of iron and copper. These matts are roasted several times, and resmelted with scoriæ and quartz, but in less proportions than at first. By these operations are obtained black copper, matts rich in copper, and the scoriæ analysed No. 2. In all these fusions no oxide of iron is reduced*.

Matts roasted repeatedly, and resmelted with scoriæ and quartz.

The

* Most of the ores and products of the smelting have been subjected to analysis, and assayed in the dry way. The following are some of the results.

Analysis of the poor ore.

The poor ore, called at Chessy meagre ore, is separated from what they call the yellow ore, which is much more rich, and is roasted and smelted separately. The poor alone is considered here. The crude ore roasted by itself, or in the great, most frequently yields no copper on assaying. Once with four parts of black flux Mr. Gueniveau obtained from it 1¼ per cent of tolerably pure copper. By chemical analysis it never afforded him more than 4 or 5 per cent of copper; but it contained from 36 to 55 of iron, according as it was mixed with the gangue, or more or less roasted. The mean of the iron may be estimated at 40 per cent.

Matts.

The matts obtained from the first smelting of the ore amount to seven hundredths of its weight. They contain about 25 parts of sulphur, 40 of metallic iron, and from 25 to 27 of copper. It is of importance to observe, that of 45 parts of iron introduced into the furnace not above 5 or 4 remain in the matt after the first fusion. The whole of the zinc is volatilised in this operation.

Black copper and rich matts.

The matts are roasted and resmelted, and the products are black copper, containing 1 or 2 per cent of metallic iron; and rich matts, containing

The mixture of quartz with the ore deserves particular ce for its importance. Many trials have been made to her earthy substances in its stead, but they were with- cess; and if a vein of quartz had not been discovered neighbourhood, the working of this poor ore must en given up. The use of quartz essential.

Of the component parts of the ore, the sulphur is separated in great part by roasting, and the zinc sublimes at the first smelting: but the iron, the proportion of which in the metallic state amounts to at least 40 per cent of the mass to be fused, is unquestionably the most embarrassing for its quantity, and because by being reduced it choaks up the furnace, and stops the operation. The intention of adding quartz is to carry the iron out of the furnace. In fact the analyses above given show, that a combination takes place between the silex and oxide of iron; and that this homogeneous compound becomes sufficiently fluid in the heat of the furnace, to allow the copper and the matts to separate from the mere difference of specific gravity; so that itself can be taken off the basin of the fore hearth during the fusion. The sulphur separated by roasting, and the zinc sublimed. The iron most troublesome. Its oxide combines with the silex.

The affinity between the parts of this compound must be pretty strong, to prevent the reduction of the oxide of iron, and to enable it to yield its sulphur to the copper, which does not happen but through some particular circumstance, as in this case the presence of silex. It is an established fact in metallurgy, that iron takes sulphur from copper: here on the contrary the iron yields the sulphur to the copper, while itself unites with a compound in which it is already abundant to form the scoriæ*, and does not appear in the Their affinity pretty strong. Iron here yields sulphur to copper from a double affinity.

taining about 21 parts sulphur, 66 copper, and 8 metallic iron. It appears from various calculations, that not above a hundredth of the iron in the roasted ore remains in these two products, while they contain nearly half of the copper, that was in such small proportion.

The iron therefore has been separated and carried off in the scoriæ, since no other product retains any considerable quantity. Iron carried off in the scoriæ.

The sublimed matter, or cadmia of the furnaces, is nothing but oxide of zinc with some slight traces of copper, and free from iron. Zinc.

* Beside the quartz and scoriæ presumed to retain copper, scoriæ from the smelting of the ore, containing about 0·66 of oxide of iron only (analysis No.

the rich matts, except in quantity sufficient as it were to complete the saturation of the sulphur.

A small quantity of copper extracted from a large portion of iron.

The process adopted at Chessy is as remarkable for the manner in which it accomplishes its object, as for the simplicity of the agents it employs. From a ferruginous mass containing only 4 or 5 per cent of copper, more than half this is obtained; a result, that theory could scarcely have predicted, since it can hardly render it conceivable.

Oxide of iron combined directly with silex.

To ascertain how far oxide of iron is capable of direct combination with silex, Mr. Gueniveau intimately mixed some red oxide with powdered quartz; put the mixture into a crucible lined with a thin stratum of levigated quartz; and exposed it to a forge fire equal to the heat of the furnace.

Forms a glass,

The first trials produced only glasses more or less opake and coloured; but at length he formed a compact compound of a metallic aspect, exhibiting laminæ indicative of crystallization, and in fact perfectly similar to the scoriæ. The coating of the crucible was attacked, but the crucible was not melted. When the silex was equal or superior in

unless the iron be predominant.

weight to the oxide, the product was vitreous; when it was only a third, fourth, or sixth of the mass, it was compact and metallic.

The operation imitated in the small way.

He likewise attempted to produce in a crucible the same effects as took place in the furnaces of Chessy. Accordingly he mixed with great care some of the rich copper pyrites, as well roasted as possible, with 14 hundredths of its weight of pure silex, made it into a paste with olive oil, and put it into a crucible not lined. On heating it as in a common assay, a portion of red copper and some grains were obtained. Some matts were distinguishable, but the greater part of the substance covering the copper had precisely the appearance of the scoriæ of Chessy.

Why does not silex unite with oxide of iron in iron furnaces?

Perhaps it may be asked why in iron furnaces, where an ore mixed with quartz is frequently smelted, the same combination between the silex and iron is not formed? For this several causes may be assigned, acting either separately or

No. 1), are added to the roasted matts; and the scoriæ issuing from the furnace during this operation contain three fourths of their weight (analysis No. 2).

conjointly.

conjointly. In the first place no doubt the high temperature of these furnaces, and the long time the ore is kept in them, must be reckoned*; neither must we omit the presence of earths and metallic oxides, the action of which on the silex may counterbalance that of the oxide of iron. To confirm this opinion, Mr. Gueniveau took an artificial compound of silex and iron, added $\frac{1}{4}$ its weight of very pure white marble, made the mixture into a paste with olive oil, and placed it in a crucible lined with charcoal powder. A button of iron and a few grains were obtained; and the scoriæ were an opake glass, of a light green colour, that did not contain a fifth of the iron. An analysis of the scoriæ of the high furnace shows, that the separation of the iron is much more complete, when lime, alumine, and oxide of manganese are found in due proportion with the silex.

Owing to the high temperature, length of the operation, and presence of other substances.

Lime, alumine, and manganese, promote the separation of the silex.

The presence of other earths beside silex, which is advantageous in iron furnaces, because they facilitate the reduction of this metal, is for the same reason prejudicial in furnaces for smelting cupreous pyrites. In these furnaces opposite effects are endeavoured to be produced on the iron: the object being to reduce the oxide of the metal in the former; while in the latter it is to prevent this reduction, and at the same time to promote the reduction of the copper. The means employed by the metallurgist to effect these opposite purposes merit the attention of the man of science.

These earths, advantageous in one case, prejudicial in the other.

The scoriæ produced in the smelting works of copper pyrites resemble iron ores, not only in their aspect and magnetic properties, but in the facility with which they yield a very considerable quantity of iron when assayed. It may be reckoned, that the mean produce of the assay would be at least 40 per cent; a quantity superior to that of many bog iron ores, which are notwithstanding wrought with advantage.

The scoriæ more rich in iron than many iron ores.

To ascertain how far these scoriæ are capable of being wrought for iron in the high furnace, they were treated with 25 per cent of carbonate of lime, and a well united button was obtained, equivalent to two thirds of the iron it contained.

Examined with a view to working them.

* This is an opinion long held by Mr. Hassenfratz.

Mr.

May be smelted with profit.

Mr. Gueniveau thinks therefore, that they may be smelted like the bog iron ores with an argillaceous carbonate of lime; and though the iron produced might not be of prime quality, it would answer for several purposes. This must appear an object of considerable importance to those, who have seen the vast quantities of scoriæ near the works. The only operations required are pounding and smelting; and the smelting would not be very expensive, if coke could be employed, as is done at Chessy for the copper, where the whole process is conducted in a very intelligent and economical manner.

Silex acts chemically in this case:

The observations and experiments here given not only lead us to consider silex, or quartz, as a metallurgic agent, capable of separating iron in a state of combination from copper, which it allows to melt alone or mixed with sulphur, when assisted by the high temperature of the furnace; but serves to explain many passages in treatises on metallurgy. These works offer nothing precise respecting the manner, in which substances mixed or combined in ores act on one another, and allow the separation of the metal, which we seek to obtain. The same facts teach us why various kinds of scoriæ are mixed with the ores or matts under different circumstances: some being employed as fluxes of the gangues; others only to give fluidity to the whole mass, and produce a kind of solution, which facilitates their reciprocal action; and others act as a medium to separate the iron, when they are not already saturated with it. At Chessy the scoriæ of the ore perform this office in the smelting of the matts, as has been said. The general opinion of metallurgists therefore, who consider the addition of earthy substances to ores as serving merely to form fusible compounds with their gangues, without paying any attention to the action they exert on metallic oxides, requires modification in many cases.

which elucidates many things that occur in metallurgic works.

VIII.

Description of the Valve Siphon of the late Mr. AMI ARGAND, *Inventor of the Lamps with a Double Current of Air**.

THIS improvement, though simple, is ingenious, and particularly adapted to large siphons, that require to be removed from one vessel to another. A valve, as E, or H, pl. II, fig. 1, is applied to the foot of the shorter or ascending leg of a siphon A B, B C, at the other foot of which a stop cock F is placed. The cock being open, and the foot E immersed in any liquid in a vessel I K, by moving the leg E perpendicularly downward and upward, the liquid will gradually ascend through the valve E, till it runs out at the point L. The pressure of the air on the surface I will then be sufficient, to force the liquid through the valve E, as long as this remains beneath it; and thus it will continue to act as a common siphon, and the vessel will be emptied, unless supplied from some reservoir, as N. Description of a valve siphon.

As soon as the siphon is filled, and begins to discharge the liquid at L; or at any period while it continues full; if the cock F be turned so as to stop it, it may be very safely and conveniently removed to any other vessel; as the cock will prevent the liquid from running out at one end, and the valve at the other: and the moment the extremity E is immersed in the liquid in another vessel, and the stop cock F turned, it will act again as before. Conveniently removable.

The siphon may be filled in this way in a clear liquid, and then removed into a vessel of the same kind of liquid, that has a sediment at bottom, which would be disturbed by moving it up and down. This however may not always be convenient: Mr. Argand therefore makes an aperture with a short perpendicular tube O in the horizontal branch B B, through which, by means of a funnel, D, the siphon may be filled, while the cock F is shut; so that it may be inserted into the liquid, and made to act without disturbing it. When Mode of filling without disturbing the sediment of liquids.

* Sonnini's Bibliothèque Physico-économique, Nov. 1806, p. 117.

the

the siphon is thus filled, or when the funnel D is not required, the aperture at O is closed by the stopple G.

Made to take to pieces.

For the convenience of carrying the siphon, as well as for packing it up, or cleaning it, the horizontal and perpendicular branches are made to take asunder at the joints M M. The nozzle L is likewise made to take off, as it is frequently more convenient for the fluid to be drawn off perpendicularly.

IX.

Description of a new Instrument for proving the Strength of Gunpowder, by Mr. Regnier, *Keeper of the central Depot of Artillery**.

Defects of the old spring instrument.

THE old toothed wheel instrument for trying powder is obviously defective, as the friction of the spring blunts and rounds the edges of the teeth; the spring, being necessarily strong, and always in a state of tension, loses in time its strength and elasticity: and the wheel, which is too much confined in its movements, is exposed to irregular degrees of friction, which likewise vary according as the instrument is kept clean, or suffered to grow rusty. To obviate these defects Mr. Regnier has recourse to a weight; and the following is a description of the mode in which he applies it.

A weight here employed.

Decription of the instrument.

A, plate II, fig. 2, is a stock of walnut wood, about 8 inches long, with a plate of copper let into it, to support the mechanism.

B, a small copper mortar, to hold the powder to be tried, with a pan for firing it.

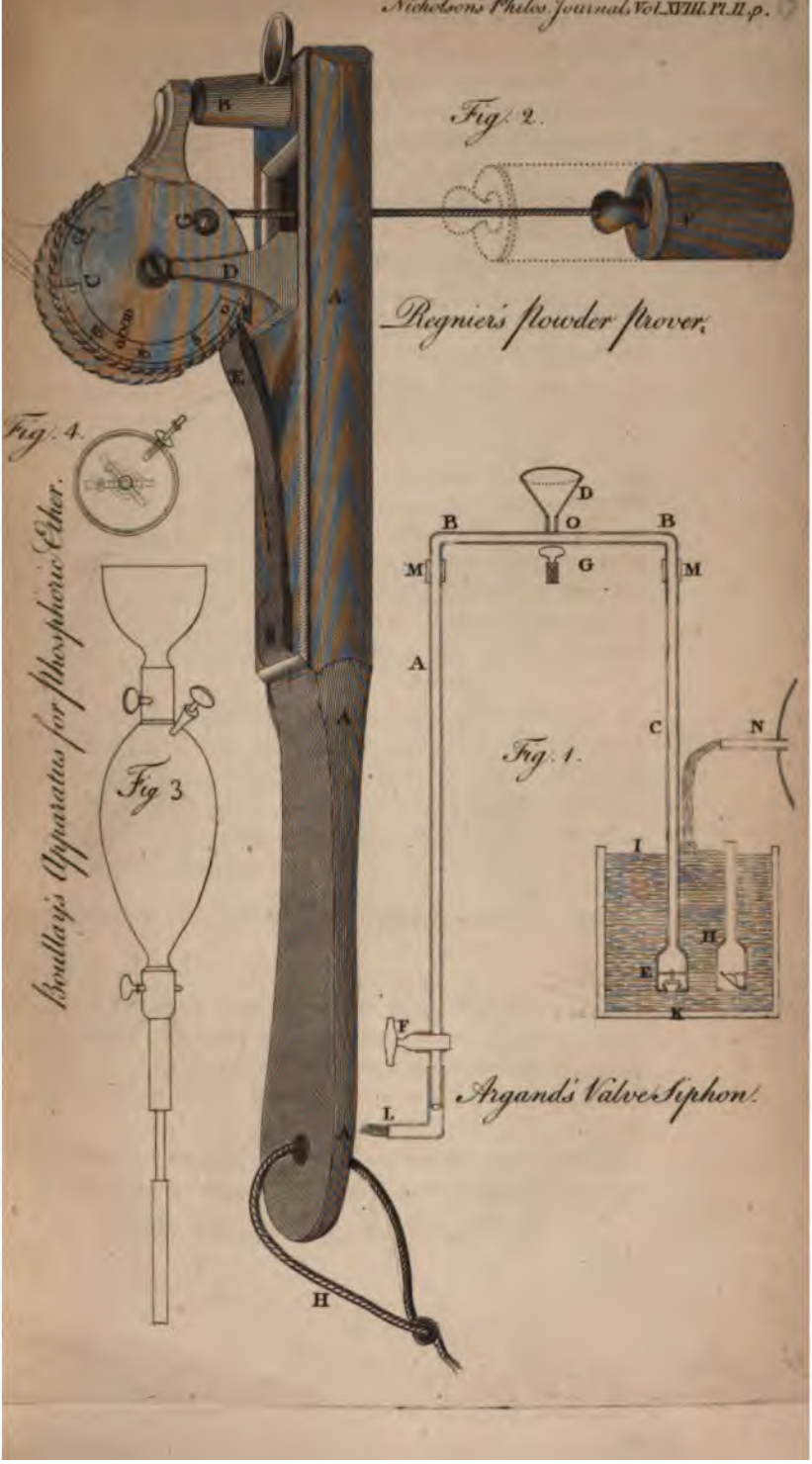
C, a brass wheel, grooved like the wheel of a pulley, with thirty ratchet teeth on its circumference. To this wheel is fixed a projecting piece, or obturator, accurately covering the mouth of the small mortar, B.

D, a forked support, in which the wheel C plays freely on its axis.

E, a spring pallet, acting as a click, to stop the wheel at the point to which the strength of the powder carries it.

* Sonnini's Bibliothèque, March, 1807, p. 415.

Nicholson's Philos. Journal, Vol. XVIII. Pl. II. p. 6



THE
PUBLIC
ASTOR, LENOX AND
TILDEN FOUNDATIONS

F, a small copper weight suspended by a string, which is fastened by a knot at the hole G in the thickness of the wheel. The string passes through an aperture in the wooden stock and the plate of copper, sufficiently large to occasion no friction.

H, a loop at the end of the handle, to hang up the instrument when not in use.

To use it the small mortar B is filled with powder, and the end of the finger passed over its mouth, that no grains may remain between the mortar and its obturator. A little priming is to be put into the pan, and the instrument is to be held in a horizontal position in the left hand, while it is fired. A red hot iron skewer is the most convenient for setting fire to the priming. Method of using it.

The elastic fluids extricated on firing the gunpowder impel the obturator upward, thus turning round the wheel, and raising the weight, that resists its action. The strength of this action is indicated by the extremity of the pallet E, that marks the degree to which the weight is raised. Its mode of action.

The degree marked *good* denotes a good powder for shooting; but the farther it goes beyond this, the better the powder.

The same powder will not always produce an equal effect on the same instrument; to prove it properly therefore several trials should be made, and the mean of them taken. To prove the large grained powder used for military purposes other modes must be adopted, this instrument being suited only to the sportsman. Adapted to the purposes of the sportsman only.

X.

Mode of making Phosphoric Ether by means of a peculiar Apparatus; by Mr. P. F. G. BOULLAY, *Apothecary, at Paris. Read before the First Class of the Institute, March the 23d,* 1807*.

BOTH Scheele and Lavoisier had attempted to transform alcohol into ether by means of the phosphoric acid, without Unsuccessful attempts to make phosphoric ether.

* Annales de Chimie, vol. lxii, p. 192, May, 1807.

without success, when Boudet the younger published a paper on this subject in the XLth vol. of the Annales de Chimie. The phenomena he described announce a real action between the acid and alcohol, and display various circumstances, that usually accompany the formation of ether. However this chemist confessed, that the product he obtained possessed little volatility; that it was entirely soluble in water; and that, though it had a peculiar smell, it did not exhibit the characters of real ether.

Owing to the difficulty of bringing the acid & alcohol into intimate contact.

Convinced by various trials, that the want of action of the phosphoric acid, when concentrated or even glacial, on alcohol depended particularly on the difficulty of uniting these two substances, and multiplying and prolonging the contact of their mutual particles, I resumed the attempt; and the hope I conceived of obtaining a more satisfactory result was realized by the following process.

Apparatus for this purpose.

To a tubulated retort, placed on a sand bath, I fitted a receiver, likewise tubulated, which communicated by a Welter's tube with a bottle full of limewater. From this bottle a second tube proceeded to a pneumatic trough, and there opened under an inverted jar.

Into the retort I introduced 500 gram. [1603 grs. troy] of pure phosphoric acid, arising from the combustion of phosphorus by nitric acid, vitrified, redissolved, and evaporated to the consistence of honey.

On the tubulure of the retort I placed a glass vessel, that may be called a *reservoir*, of an oblong shape, and open at both extremities, each of which might be shut close by means of a cork. From the lower end a tube descended to the bottom of the retort, being thus immersed in the phosphoric acid. The upper end, in which was a funnel, that might be made to communicate with the reservoir, or not, at pleasure, had a small aperture with a ground glass stopple intended to give vent to the air, when displaced by pouring in a liquid. See plate II, fig. 3; and for its horizontal section fig. 4.

Process.

The apparatus being thus disposed and carefully luted, and the first receiver being cooled by a mixture of pounded ice and common salt, a fire was kindled under the retort, and the heat gradually increased, so as to heat the acid to

95°

95° of Reaumur [240° F.]. An equal weight of alcohol at 40° was then introduced into the reservoir, and by means of the lower cock allowed to fall drop by drop into the hot and fluid phosphoric acid. The mixture took place with violence and ebullition; it assumed a black colour, and copious streaks immediately appeared on the upper part and neck of the retort.

The fire being kept up, and the distillation continued to dryness, there passed into the receiver, Products.

1, A hundred and twenty gram. [3oz. 6dr. 53gr.] of alcohol weakly etherised. Alcohol slightly etherised.

2, Two hundred and sixty gram. [8oz. 2dr. 55gr.] of a white, light fluid, of a brisk smell, and much more ethereal than the former. A more ethereal fluid.

3, Sixty gram. [1oz. 7dr. 26gr.] of water saturated with ether, on which swam about 4 gram. [62grs.] of a lemon-coloured fluid, with an empyreumatic smell, very similar to that which comes after sulphuric ether, and which is commonly known by the name of sweet oil of wine. Water saturated with ether, and oil of wine.

4, Another fluid of an insupportably fetid smell, reddening tincture of litmus, and combining with carbonate of potash with effervescence. This combination, being evaporated to dryness, was a deliquescent salt, foliated, and perfectly similar to acetate of potash. A fetid liquor with acetate of potash.

The limewater was rendered turbid, but not till toward the end of the distillation. A little carbonic acid at the end.

Beside the air in the vessels, a gas was collected of a sweet and penetrating smell, burning with a white flame, and when burned depositing on the sides of the vessel a very plentiful coat of carbonaceous matter. It was a little ether that escaped condensation, that passed at the same time as the most ethereal liquid product, and a little before the white vapours, that announced the presence of the oil. Ether in the state of gas.

What remained in the retort was a blackish, glassy matter, consisting of phosphoric acid, and a little charcoal. Residuum.

The first two products mixed together, and rectified over dried muriate of lime, at a heat of about 50° [144°], afforded near 60 gram. [1oz. 7dr. 27gr.] of a liquid, which in smell and taste had the greatest resemblance to the purest sulphuric ether. It marked like it 60° on Baumé's areometer, the Rectified. Pure ether.

thermometer being at 10° [54½°]; dissolved in eight or ten parts of cold water; evaporated quickly; boiled at 30° [99½°]; dissolved resins and phosphorus; burned with a whitish flame, leaving a carbonaceous residuum, and without any trace of acid appearing from its combustion on the suface of water.

Alcohol slightly etherised.

The other product of the rectification was alcohol, slightly etherised. This alcohol, passed again in the same manner through the phosphoric acid employed in the experiment,

More ether.

gave rise to the formation of a fresh quantity of ether in every respect resembling the first.

From these facts, and on examination of the products submitted to the inspection of the class, it appears to me,

Phosphoric ether

1st, That phosphoric acid is capable of transforming alcohol into a perfect ether, by means of the apparatus I employed, and the precautions I have mentioned:

most resembling sulphuric.

2dly, That, of all the different ethers known, the ether resulting from the action of phosphoric acid on alcohol has the greatest analogy to sulphuric ether, with respect to its properties, and the phenomena observed in preparing it.

XI.

Remarks on the Decomposition of Acetate of Barytes by Means of Soda; by Mr. D'Arcet*.

Acetate of soda said to be decomposed by barytes.

In N°. 180 of the Annales de Chimie, p. 286, Mr. Perpères, speaking of the formation of acetous acid in cases of indigestion, says, that, to ascertain the presence of this acid, he saturated it with pure soda; afterward decomposed the acetate of soda by means of barytes; and having thus set the soda free, dissolved it in alcohol, which, uniting with the water of the solution, precipitated the acetate of barytes, that had been formed. The result of this experiment is certainly inaccurate, as the following details will show.

Solution of barytes added to acetate of soda, crystals fall down.

Take a hot saturated solution of barytes, pour it into acetate of soda, and immediately an infinite number of little, shining, and iridescent laminæ will fall down. If these

* Annales de Chimie, Vol. LXI, p. 248, March 1807.

be

be separated from the liquid after it is completely cooled, washed in the smallest possible quantity of water, and dried quickly by pressing them between several folds of blotting paper, they will be found to be crystals of pure barytes, without any mixture of acetate. Of this I satisfied myself in the following way. These pure barytes.

1. I exposed part of these crystals to the air. After some days I washed the carbonate thus obtained with pure water, which then took up nothing, that sulphuric acid, or alkaline carbonates or sulphates would throw down. The whole of the crystals therefore had been converted into carbonate; which would not have been the case, had they contained any acetate of barytes. Converted into carbonate by exposure to the air.

2. I dissolved two or three grammes of the same crystals in distilled water. The solution restored the blue colour of reddened litmus paper: consequently it contained an excess of alkali. Were alkaline.

I added a few drops of sulphuric acid to this solution, and a precipitate of sulphate of barytes was formed. I tested the liquid again with litmus paper, and I still found an excess of alkali. I then gradually added more sulphuric acid, till there was a slight excess of acid in the liquor; filtered it, and found it no longer contained any barytes, but a little free sulphuric acid. This would not have been the case, had the crystals contained any acetate of barytes; for, on this supposition, the moment when the excess of acid began to be sensible by the test paper, only a small portion of the acetate would have been decomposed, and acetous acid would have been set free. The filtered liquor therefore ought to have contained a slight excess of acetous acid, and the undecomposed acetate of barytes: but this was contradicted by the experiment. Sulphuric acid formed with them sulphate of barytes, and no acetous acid appeared.

3. The mother water of the crystals employed in the preceding experiments ought to contain only that small quantity of pure barytes, which it could retain in solution when cold, in addition to the whole of the acetate of soda employed. This too the analysis of the mother water demonstrates, if alcohol be poured into it, as Mr. Perpérès directs. The shining scales that fall down are nothing but crystals of barytes: and examined in the way I have mentioned The mother water contained only pure barytes and acetate of soda.

above, they produced nothing but very pure carbonate of barytes, and not an atom of acetate. If the mother-water be further examined with sulphuric acid, or alkaline carbonates, it will immediately appear, that it contains but little barytes and a great deal of acetous acid; which becomes still more sensible, if it be evaporated to dryness, and the residuum be redissolved in distilled water: for this solution does not contain an atom of barytes, but merely acetate of soda; the little barytes that was present being reduced to the state of a carbonate during the evaporation.

Barytes does not decompose acetate of soda.

Hence it follows, that barytes does not decompose acetate of soda; and on the contrary, that, if we try the opposite experiment, it will succeed. In fact the whole of the acetate of barytes may be decomposed, by adding to it a sufficient quantity of pure soda to saturate all the acetous acid. The barytes contained in the phial accompanying my letter was prepared in this way.

My object is not to invalidate the conclusion of Mr. Perpérès, which appears to me just, and consistent with what was already known. I only criticise one of the proofs he has adduced, and avail myself of this opportunity, to remind the public, that in the year 12 a paper on the affinities of barytes, by Mr. Anfrye and myself, was inserted in the Annales de Chimie; where we proved, that in the classification of alkalis barytes ought to be placed before potash and soda only with respect to the sulphuric and carbonic acids, the affinities of potash and soda being superior in every other case. How is it, that, notwithstanding the facts so positively announced in that paper, different authors have retained the ancient order of affinity assigned to barytes? It appears to me, that, with regard to experiments, either the results of such as are made public should be adopted; or they should be refuted, by repeating them, and proving their erroneousness.

Potash and soda have stronger affinities than barytes for all the acids except the sulphuric and carbonic.

I shall conclude this note by citing in confirmation one of the processes, the goodness of which has been proved by our labours on barytes in the large way. It follows naturally from the facts mentioned above.

This theory applied to the process of ob-

The decomposition of muriate, nitrate, and acetate of barytes by potash or soda is so complete and easy, that it is

is unquestionably the most simple method of procuring in a laboratory the barytes that may be wanted. For this purpose a hundred parts of sulphate of barytes accurately mixed with twenty parts of charcoal powder are to be calcined by a strong heat in close vessels. After being exposed to a high heat for an hour, the crucible is to be suffered to cool; the residuum separated and diluted in water; and a sufficient quantity of nitric, muriatic, or acetous acid added. The mixture is to be heated gently, when it will give out a large quantity of sulphuretted hidrogen and carbonic acid, which must be guarded against with care. When the effervescence ceases, and test paper indicates a slight excess of acid in the liquor, it is to be filtered and evaporated, to decompose the sulphuretted hidrogen, and precipitate the sulphur, that was retained in solution*. The residuum is to be redissolved in the least water possible, and a saturated solution of caustic potash is to be added. At the instant of mixture a large quantity of crystals of barytes falls down. The whole being left at rest in as low a temperature as possible for an hour or two, the mother water is to be poured off; the crystals are to be washed with a little distilled water, and then dried by pressing them between folds of blotting paper; and lastly they are to be dissolved in as much boiling water as is necessary. This solution, being filtered, will let fall when cold the barytes, which is much more pure, and costs less, than when obtained from the decomposition of nitrate of barytes by heat alone.

taining pure barytes in the large way with success.

It is to be observed, that the muriatic or acetous acid is preferable to the nitric, because each forms a more soluble salt than the nitric, and the washing is more easy; and because in making the solution the nitric acid is partly decomposed, and oxigenizes a portion of the sulphuret of barytes, so that some of the acid is lost, and some of the barytes absorbed by the sulphuric acid formed.

Muriatic or acetous acid to be preferred.

The caustic potash used in this process must be prepared from carbonate perfectly free from sulphate.

The potash must be free from sulphate

* The same object may be attained more readily by pouring into the liquor a few drops of solution of nitrate of copper or lead, letting the metallic sulphuret subside, filtering afresh, &c.

Observation

Observation on the preceding article by one of the authors of the Annales de Chemie.

ıe result examined by ıyton.

Agreeably to the request of Mr. d'Arcet, in addressing to me this note, I have examined the liquid in the phial accompanying it. It was more than half full of small, white, crystalline scales. The liquor acted powerfully on paper tinged by mallow flowers, changing it green. On dropping into it sulphuric acid a little in excess, a copious precipitate of sulphate of barytes was formed, without the least smell of acetic acid. After having filtered the liquor from this precipitate, I evaporated it by a gentle heat, in a platina capsule; but it left no trace of any neutral salt. No doubt can remain therefore, but the acetate of barytes is radically decomposed by soda.

L. B. GUYTON.

SCIENTIFIC NEWS.

Adjudication of Prizes, with a proposed new Question, by the Imperial Academy of Sciences at St. Petersburgh.

ıuestion proosed by the ıademy on ght.

THE Imperial Academy of Sciences had proposed in their last public notice the prize of five hundred rubles [£112 10*s.*], to be given to any professor of natural philosophy, who would establish, and communicate to the academy, a series of "new and instructive experiments on light "considered as matter; also, on the properties, which may "in part be attributed to it; on the affinities, which it may "appear to have either on organized or unorganized bodies; "and upon the modifications and phenomena of these substances by their combinations with the matter of light." —The academy had declared at the same time, in order not to confine the learned who might have been pursuing similar inquiries, that they contented themselves with stating the subject generally, leaving them at liberty to consider the question in any point of view, that might appear best calculated to elucidate a question so difficult.

The academy has received within the prescribed time six tracts on the question, each having a note sealed and motto, —viz.

—viz. No. 1. In the Russian language with the motto: "*A "philosopher who has learned to doubt, knows more than all "the learned, &c.*" No. 2. In the Russian language: "*Time is the earliest thing in nature, &c.*" No. 3. In Latin: "*Est-ne color proprius verum, lucisne repulsus eludunt "aciem?*" No. 4. In French: "*Nox abiit, nec tamen orta "dies!*" No. 5. In German: "*Ut noscas splendore novo "res semper egere, et primum factum, &c.*" No. 6. In German: "*La physique ne sera véritablement une science, "que lorsque tous les effets naturels se deduiront clairement "d'un seul et meme principe evidemment démontré.*"

The first three tracts, beside the common fault of wanting new experiments, a complete and instructive series of which was required by the academical notice, contained hypotheses and propositions, either well known, erroneous, or ill expressed, and advanced without demonstration. For these reasons the academy did not think they had any claim to the prize.

The tract No. 4 is not without merit. The author enters upon several interesting questions concerning the nature of light, in a manner that readily convinces us he is no stranger to the subject. But the deficiency of connexion and systematic arrangement, which is perceived in it, and above all, the absolute want of new experiments which might lead to new results, or serve as a support to a number of hypotheses advanced by the author, and destitute of every species of demonstration, would not permit the academy to adjudge the prize to this memoir, even had there been none of greater merit.

As to the last pieces, No. 5 and No. 6, the academy has found them worthy of their particular attention, from the report of the committee appointed to decide on the different performances. These essays are agreeable to the principal condition stated in the notice, inasmuch as they contain a great number of new experiments on the effects and properties of light, and a judicious application of those, which, though already known, were repeated, whenever they appeared doubtful to the authors. Both pieces are executed upon a plan wisely conceived, expressed with clearness, and arranged in sufficiently systematic order. On the other hand,

hand, in each were found some incoherent and contradictory conclusions; as also propositions advanced without sufficient proof; beside some errours, and obscure passages. But as these imperfections were overbalanced by researches of great merit, the academy, without acceding to every assertion of the authors, have nevertheless thought it their duty, to divide the prize between them, thinking them worthy of encouragement and honourable reward.

On opening the two sealed notes, Doctor Henry Frederick Link, professor of natural philosophy at the university of Rostock, was found to be the author of No. 5; and Mr. Placidus Heinrich, professor of natural philosophy and mathematics at the Abbey of St. Emereau, at Ratisbon, the author of No. 6. The notes of the remaining tracts were burned, without being opened.

Question on the resistance of fluids.

When the academy had made public the notice, in which the marine department proposed a prize on the question concerning the resistance of fluids, they had engaged to publish also the judgment, which that department, in conjunction with the academy, should make on the memoirs presented. Conformably to this engagement, the academy announce the receipt of three memoirs:—viz. No. 1. with the motto: "*Sit modus lasso maris et viarum militiaeque.*" No. 2. "*Praesta natura voce doceri, quam ingenio suo sa-*"*pere.* No. 3. "*England and France agree.*" The last of these arrived after the term.

Neither of them was found to satisfy all the conditions of the problem: but, as the tract No. 2 exhibits a new theory; which, though not established upon grounds sufficiently solid, or applied to naval architecture in the manner the notice required, is preferable, in some measure, to the theories of Romé and don George Juan, agrees better with experiments than the common theory, and deserves therefore to be noticed advantageously; the marine department, to recompense the author for his trouble and laudable efforts, have decreed to him the prize of 100 Dutch ducats [£46 5*s.*], and the academy have given their sanction to the decision. The opening of the sealed note discovered the author to be Mr. Zachary Nordmark, professor of mathematics in the university of Upsal.

In

In publishing these judgments and distribution of prizes for the year 1806, the academy proposes the following question for the present year 1807.

Question for 1807.

Chemistry teaches us the means of discovering the noxious quality of mineral bodies, whereas it is only empirically, that we have learned to distinguish poisonous plants from those that are not so. Even the characteristics, by which we think ourselves enabled to determine on the presence or absence of poison in vegetables, are not always sufficiently certain and incontestible. The livid colour, for example, which has rendered many plants suspected, is a fallacious sign. The burdock (arctium lappa) looks dull, and is of a pale colour, yet it is a wholesome plant; on the contrary, the laurel (daphne) is remarkable for the beauty of its flowers and leaves, yet this is poisonous. The families of ranunculus and anemone are as beautiful as they are numerous; they are however for the greater part noxious. The same may be said of the disagreeable smell of plants, which is taken for a diagnostic of the poisonous quality, and which sign is equally uncertain with the preceding. The smell of the laurel is very agreeable, while the stinking orach (chenopodium vulvaria), an innocent and even salutary plant, is of a very disagreeable smell. The smell of coriander is disagreeable to many persons, yet it is of a very salutary nature. The umbelliferous plants, that grow in wet and marshy situations, have the reputation of being poisonous; notwithstanding this, the water parsnep *(sium)* and all its species, the *sison inundatum et sulsum*, the *phellandrium aquaticum*, the *angelica sylvestris*, the *ægopodium podagraria*, plants which thrive in marshes, contain no poison. It is plain, therefore, that neither the livid colour, disagreeable smell, nor growth in marshy places, can furnish us with certain and indisputable signs of the presence of poison in plants.

The pretended repugnance of animals to pernicious plants is evidently as little infallible. The division of plants, made by botanists, into classes, orders, and families, according to their nature, is not more effectual in discriminating those that are venomous. To be convinced of this we have only to observe, that among the species of the nightshade, a genus so much suspected, are found the potato *(solanum*

(solanum tuberosum), and the winter cherry *(s. pseudocapsicum)*, which possesses the virtues of a stimulant, and of destroying the pernicious principle in narcotic plants.

In consequence of this want of an exterior and natural certain sign, by which poisonous plants might be immediately detected, it would be desirable, to find out some easy method of examining them, such for instance as a kind of *eudiometer*, or any thing that might produce changes in them, which, like the black colour assumed by mushrooms when they are boiling, might indicate their noxious qualities; though it must be confessed, the criterion of poisonous mushrooms is not yet sufficiently established.

"*An easy method is therefore required, by which any individual, not having the least knowledge of botany, may detect poisonous plants, in a short time, at a small expense, and in a manner perfectly decisive.*"

The prize is one hundred Dutch ducats [£46 5*s.*] and the precise time, after which no memoir can be admitted to the competition, is the 1st of July, 1808.

The academy invites the learned of all nations, without excluding its honorary members and correspondents, to investigate this subject.

The learned who compete for the prize are not to put their names to their works, but merely a sentence, or motto; and send with them sealed notes, which must have the same motto on the outside, and the author's name, quality, and place of residence, within. The note of the piece to which the prize is adjudged will be opened, and the rest shall be burned unopened.

The tracts should be written in legible characters, either in Russian, French, English, German, or Latin, and must be addressed to the permanent secretary of the academy, who will deliver to the person appointed by the author a receipt, marked with the same motto and number as shall be inscribed on the piece. The successful memoir is to be the property of the academy, without whose formal permission the author shall not print it.

The rest of the tracts may be received back from the secretary, who will deliver them at St. Petersburgh to any person commissioned by the author to apply for them.

Discovery

Discovery of a new Planet, by Mr. OLBERS.

Mr. OLBERS has written to Mr. le Français Lalande, that he has recently discovered a new planet. The following are such elements of its orbit, as he has been able to determine. New planet discovered by Olbers.

The 29th of March, at 21 minutes after 8 mean time, its ascension was 184° 8′; its north declination 11° 47′. Elements of its orbit.

On the 30th, at 12 h. 33′, mean time, its ascension was 183° 52′; its north declination 11° 54′.

It has been seen at Paris, and was visible to the naked eye. Its size appears nearly that of a star of the fifth magnitude. Apparently it is about the same distance as the three lately discovered planets, Ceres, Pallas, and Juno. Seen at Paris by the naked eye.

Fluoric Acid in Teeth and Bones.

In our Journal, Vol. XIII, p. 214, is a letter with which we were favoured by Mr. Brande, to show, that the enamel of the teeth does not contain any fluoric acid, an Italian chemist having asserted, that they did. A letter from Mr. Gehlen to the editor of the Journal de Physique, dated March 16, informs us, however, that the fluoric acid exists both in the enamel and bony part of teeth, and in other bones. His words are: Fluoric acid in teeth & bones.

"The very extensive and accurate experiments of Mr. Berzelius of Stockholm have proved, that the enamel and the bony part of the teeth of man and of the ox, as well as their bones themselves, contain fluoric acid. The following are the results of the analyses.

"Enamel of human teeth.		*Enamel of the teeth of the ox.*		
Phosphate of lime	85·3		81	Component parts of the enamel of teeth,
Fluate of lime	3·2		4	
Carbonate of lime	8·0		7·1	
Phosphate of magnesia	1·5		3	
Soda, combustible animal matter, water	2	Natron Animal matter	1·34 3.56	
	100		100	

Osseous

	"Osseous part of human teeth.		Osseous part of the teeth of the ox.
of their bony part,	Phosphate of lime	61·95	57·46
	Fluate of lime	2·10	5·69
	Carbonate of lime	5·50	1·38
	Phosphate of magnesia	1·05	2·07
	Soda, with a little muriate of soda	1·40	2·40
	Gelatine, veins, water	28·00	31·00
		100	100

	"Dry fresh human bones.		Dry fresh ox bone.
and of bones.	Gelatine	32·17	33·30
	Veins belonging to their organization	1·13	
	Phosphate of lime	51·04	55·45
	Fluate of lime	2·00	2·90
	Carbonate of lime	11·30	3·85
	Phosphate of magnesia	1·16	2·05
	Soda, with a small quantity of muriate	1·20	2·45
		100	100"

We regret, that we have not the particulars of the analyses before us, that we might see the proofs of the fact, or be enabled to trace the causes of the fallacy.

Fluoric acid in urine.

Mr. Berzelius says too, in a letter to Mr. Vauquelin, that the precipitate obtained from urine by limewater, when washed and dried, being treated with sulphuric acid, gives out fluoric acid, which corrodes glass. But it requires a pretty considerable quantity of this precipitate, to give any very perceptible signs of it.

Muriatic acid and soda obtained by galvanism only when salt is present.

He adds, that the Swedish chemists have never been able to obtain muriatic acid and soda by means of the galvanic pile from water perfectly pure. That they find pure water a very bad conductor; but if the least particle of salt be present, the decomposition is more rapid, and its acid and alkali are set free.

Sulphur

Sulphur inflamed by oxide of lead.

Dr. THOMSON'S paper on the oxides of lead, Journal, Vol. VIII, p. 280, having been translated into French, and inserted in the Annales de Chimie, the passage in which he says he did not succeed, in triturating sulphur with the brown oxide, p. 288, is thus commented upon. Brown oxide of lead inflames sulphur by trituration with it.

"Nothing however is more certain, than that Mr. Vauquelin has inflamed sulphur by triturating it with brown oxide of lead, as he formerly mentioned. He lately repeated this experiment, in one of his lectures, before upwards of fifty persons, among whom was prof. Proust of Madrid. The only precautions the experiment requires are, to boil the nitric acid a long time on the brown oxide, that no minium may remain among it; to wash it afterward with a great deal of boiling water, so as to take up all the nitrate of lead; and lastly, to dry it well, and to triturate it with flowers of sulphur equally well dried. Necessary precautions.

"On observing these essential conditions, there can be no doubt, but Dr. Thomson will succeed in inflaming the sulphur. The supposition he makes, to account for the phenomenon, is inadmissible, for Mr. Vauquelin never employed oxigenized muriatic acid, to prepare the brown oxide of lead.

Yttria and Cerium.

THE chemists at Upsal at first imagined, that cerium was nothing but a mixture of barytes, yttria, and magnesia. Mr. Eckeberg, desirous of comparing them, has found that yttria, after having been a long time exposed to the action of fire, gives out oxigenized muriatic acid, when dissolved in the common muriatic acid. Is yttria, asks Mr. Berzelius, one of the new metals, uranium, titanium, or cerium, with its nature as it were changed? Yttria oxigenizes muriatic acid.

Mr. Gahn has formed an alloy of cerium with iron, partly in a grayish powder. Alloy of cerium and iron.

New

New Process proposed by Mr. ALLAIRE, *Administrator General of Forests, for scowering Wool.*

Wool cleansed by chalk.

THIS process consists in repeatedly immersing the wool in a warm chalk bath. The calcareous earth forms an animal soap with the greasy matter of the wool. By this method the wool is cleansed quickly, and without affecting its quality.

Argand's Lamps.

Argand's lamps, with blue glass chimneys.

MR. ARGAND has made different improvements in his lamps. The first consists in using blue glass chimneys, which render the light mild, like that of day: for the light traversing a medium similar to that of the atmospheric air, is modified in a similar manner. This is an important advantage to many artists, who find it necessary to work by artificial light, as it is well known, that several colours do not appear by it of their natural tints *.

The light enclosed in alabaster or Derbyshire spar.

Another mean of obtaining a very mild, pleasant, and as it were mysterious light, is to enclose the beak of the lamp in a vase of alabaster or Derbyshire spar, the bottom and neck of which are pierced to admit this beak, while the body of the lamp, that contains the oil, is concealed behind it. A room lighted in this manner has a very curious and agreeable appearance †.

Means of preventing the oil from overflowing.

Mr. Argand has likewise found the means of remedying the inconvenience to which suspended lamps are liable, that of suffering the oil to run over, either by agitation, or from rarefaction of the air in the reservoir by heat. This consists in leaving an opening in the lamp at the top, so that het temperature cannot influence it; and it will always remain at the same height,

* Chimneys made of the common blue glass of our glass houses, and of the ordinary thickness, will not answer; as I found by experience many years ago. At least they diminish the light in so great a degree, that the consumption of oil to produce a given effect with them must be so much more than with common glass chimneys, as to render them too expensive for general use. W. N.

† This is evidently analogous, but I should suppose inferior, to Count Rumford's method of using ground glass. See Journal, Vol. XIV, p. 22.

He

He has likewise contrived vessels for keeping oil, so that when the oil is drawn off, its place is supplied by water at the bottom. Thus, the vessel being always full, the oil is not thickened by the action of the air. Vessel for keeping oil without thickening.

A Course of Lectures on Natural Philosophy and the Mechanical Arts. By THOMAS YOUNG, *M. D., For. Sec. to the R. S., F. L. S., late Prof. of Nat. Phil. in the Royal Institution of Great Britain, &c. 2 vols. 4to. 1570 pages, 53 plates.*

THIS valuable work has been for some time eagerly expected; but it has suffered no-longer delay, than the copiousness of the subjects it embraces, the great variety of figures to be engraved, the large body of references, and the accuracy required in every part of it, rendered indispensably necessary. To give any adequate view of the multifarious objects it embraces, would much exceed our limits; we must be content therefore with noticing them briefly. When Dr. Y. undertook the office of professor of natural philosophy he very properly conceived, that the plan of the Institution required something more, than a mere compilation from elementary works; and therefore set himself to collect from original authors, to examine with attention, and to digest into one system every thing relating to the principles of the mechanical sciences, that could tend to the improvement of the arts subservient to the conveniences of life. In pursuing this course he has referred the fundamental doctrines of motion to simply mathematical axioms, more immediately than has been usual, and facilitated their application to practical purposes: very fully investigated the passive strength of materials of all kinds, and formed many new conclusions respecting it, of considerable importance to the engineer and architect: simplified, extended, and illustrated the theory and motions of waves, circulation of the blood, and propagation of sound: investigated the currature of the images formed by lenses and mirrors: minutely examined the functions of the eye: copiously described and accurately represented the phenomena of coloured light, and pointed out some new cases of the production of colours: reduced Young's Natural Philosophy.

reduced the theory of the tides to a simple form: investigated the cohesion and capillary action of fluids, in which he has anticipated Laplace: made various comparative experiments on the elasticity of steam, evaporation, and the indications of hygrometers: and interspersed much practical information of various kinds, with new inventions and contrivances, that would take up too much room to enumerate.

The 2d vol. commences with the mathematical elements of natural philosophy, comprehending every proposition required for forming a complete series of demonstrations, leading to every case of importance that occurs in the science, except some of the more intricate calculations of astronomy. But the greater part of it is occupied by what many will consider as not the least important of the whole, a catalogue of works relating to natural philosophy and the arts, methodically subdivided as far as could be done with convenience and accuracy. In this catalogue, works of superior merit and accuracy are distinguished by asterisks, and those the author considered as erroneous or unimportant by obelisks: beside which he has pointed out those he has quoted; and, for the convenience of those who have access to the libraries of the Royal Institution, Royal Society, Sir J. Banks, and the British Museum, the books to be found in them. Extracts to, and remarks, for the most part brief, are frequently given, pointing out the leading objects, or affording hints for farther investigation. In fact, such a body of information, and such a copious list of references to original sources, condensed into a compass comparatively so small, will not easily be found among the modern productions of our press.

Lectures on Chemistry.

Mr. ACCUM'S lectures on operative chemistry and mineralogy, exhibiting a summary exposition of the processes of experimental chemistry, and general practical rules to be observed in the performance of chemical experiments; together with a summary view of analytical mineralogy, exemplifying the practical analysis of minerals; will commence October 1st.

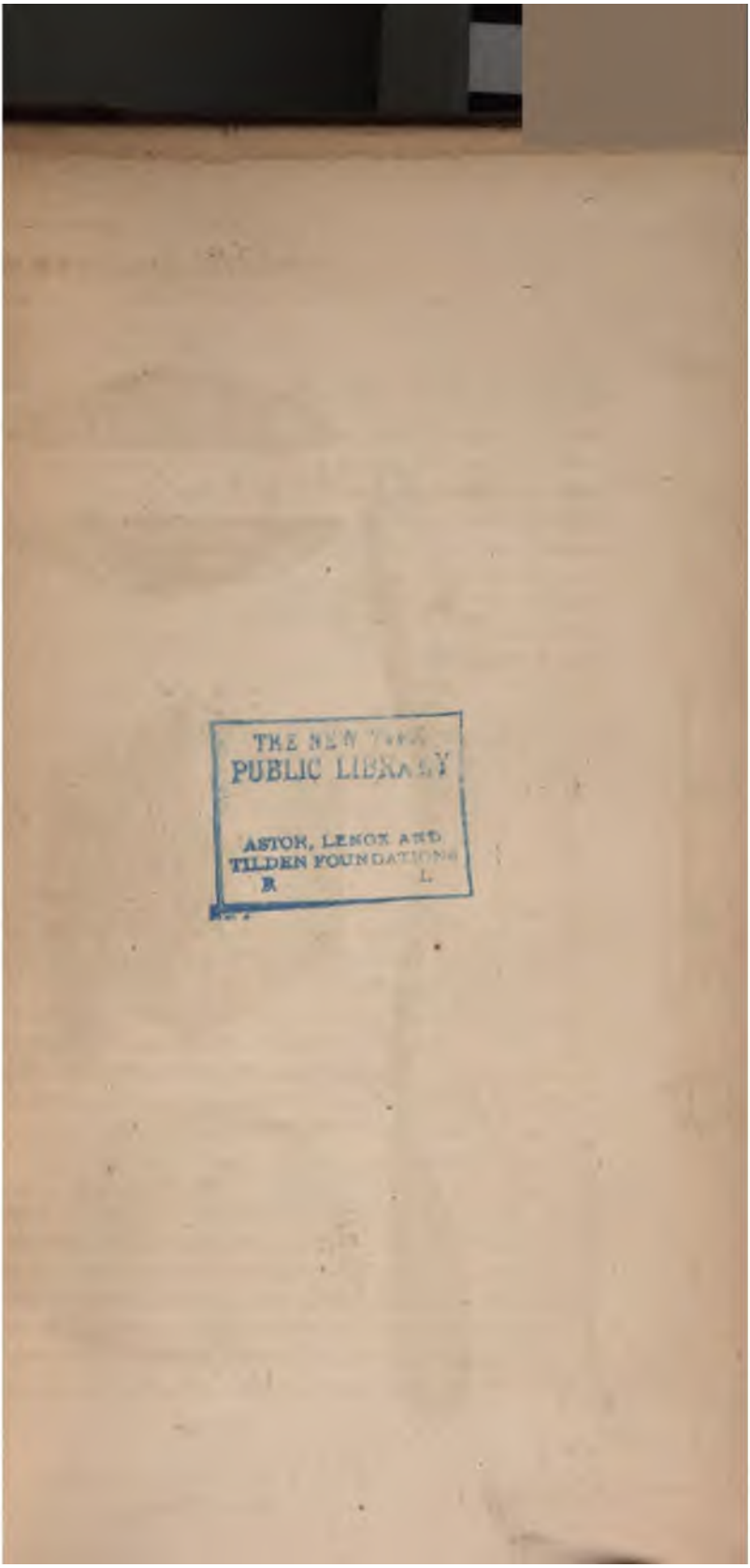
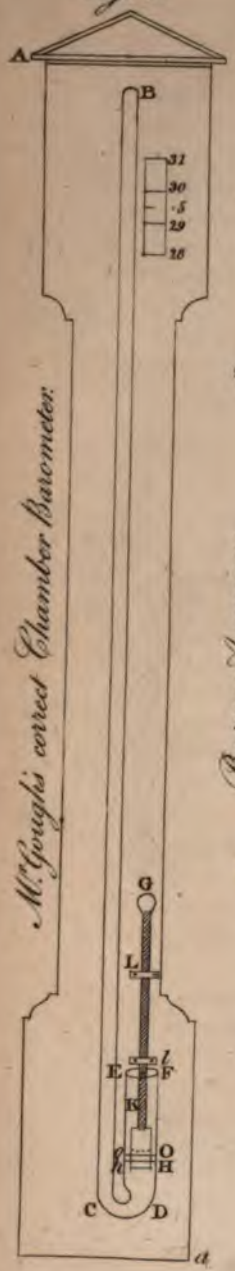

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Fig. 1.



Mr. Gough's correct Chamber Barometer.

Fig. 2.



Boteus Anguinus.

Fig. 5.



Fig. 4.



Sacro Catino.

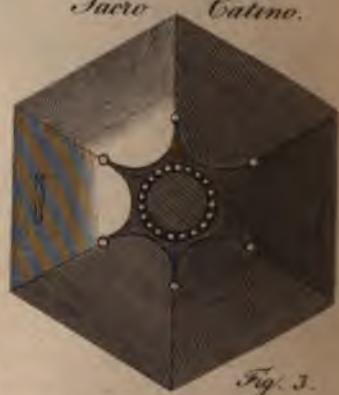


Fig. 3.



Mr. Maher's Blanching Pot.

Fig. 6.



Scale of Inches.

A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

OCTOBER, 1807.

ARTICLE I.

Description of a correct Chamber Barometer. In a Letter from JOHN GOUGH, *Esq.*

To Mr. NICHOLSON.

SIR, *Middleshaw, Aug.* 17, 1807.

MANY intelligent persons in the country have been prevented from entering upon a course of meteorological observations, by the want of good barometers; which must be procured from London, not without considerable risk. I flatter myself, the present letter will remove this objection to the study in a great measure; for it describes an easy, and perhaps original method of correcting the imperfections of the instrument, and renders it fit for the purpose of every meteorologist, whose pursuits and observations are confined to his parlour or his study. When the correction in question first occurred to me, some time ago, I communicated it to Mr. Morris, of Kendal; who made a barometer on the principle explained to him, which has been some time in my possession, and has fully answered my expectations. The above gentleman also informs me, that any artist of moderate abilities may be instructed how to construct an instrument of the same kind, by a simple diagram properly explained:

Correct barometers not easily procured in the country.

the necessary instructions will accordingly be found in the annexed figure, and description accompanying it; which explains the cause, that renders common barometers imperfect measures of the changes in the weight of the atmosphere, and at the same time points out an easy and satisfactory apparatus for counteracting this source of errour.

A correct chamber barometer described.

Suppose A a (Pl. III, fig. 1) to be the frame or outline of the barometer, the ornamental part of which may be left to the discretion of the workman: moreover let 28,31 represent a scale of 3 or more inches properly divided and furnished with a nonius: let B C D E F be the compound tube or inverted siphon containing the mercury; it is hermetically sealed at B, and open at E F; the bore of the longer leg B C is $\frac{1}{16}$ of an inch in diameter, and that of the shorter D E F $\frac{1}{26}$, in the instrument from which the description is taken. When the siphon has been filled, it is to be fixed to the frame A a, in such a manner, that the two legs B C, and D E F may have a vertical position. This being done, a circle parallel to the horizon is to be cut on the external surface of the leg D E F at the distance of 31 inches from the top of the scale, or 29$\frac{1}{2}$ inches from its middle, the place of which is denoted in the figure by the line O o. It will be perceived immediately that my barometer, as far as it has been described, differs in nothing from the common weather glass: the imperfection of which ought to be explained to the less scientific reader, before the method of correcting the instrument is described. In order to do this with the greater perspicuity, suppose, that when the tube is first filled, the surface of the mercury in the leg D E F coincides, as it ought to do, with the circle O o, and let the surface on the other leg B C fall exactly on some division of the scale, for instance 29·5: the weather glass will in this case give the true weight of the atmosphere; because the length of the column of mercury in B C is exactly 29·5 inches. But an instrument thus constructed will give the weight of the atmosphere falsely in all other instances: for let the length of the column in B C increase in consequence of an increasing pressure in the atmosphere, it is evident, that the surface of the mercury in the leg D E F will descend below the circle O o; because the augmentation in

The cause of inaccuracy in the common weather glass.

the

the opposite column must be supplied by the branch D E F; let it then descend to the station H h. Now from the construction of the weather glass here described, O o is the only base from which we can measure the length of the mercury contained at any time in B C accurately by the scale; consequently the part of the column situate in H h and O o will be neglected in the observation; and the height of the barometer will appear to be less than it really is by the space H O; that is, the top of the column will not ascend so far as it ought to do above the middle point of the scale. On the contrary, when the mercury descends in the leg B C it will rise in the leg D E F, in consequence of a quantity of this fluid coming into it from the opposite branch. As oft then as the weather glass denotes a height less than 29·5 inches, the surface of the mercury in the leg D E F will rise above O o, the true base of the scale; consequently when the column comes to be measured by the index, it will appear longer than it really is by the height of the mercury's surface in the leg D E F above the base O o; that is, the upper extremity of the column in the branch B C will fall below the point 29·5, but not so far as it ought to do. Thus it has been made evident, that the motion of the mercury in the shorter leg diminishes the range of the barometer when it comes to be estimated by the scale; which circumstance points out the necessity of a correction, and perhaps the easiest method of doing it consists in bringing the surface of the mercury in the leg D E F to a coincidence with the base O o, before setting the index attached to the scale.

The method of correcting the common weather glass by a piston.

My apparatus for this purpose is nothing more than an ivory piston K of a cylindrical figure, about $1\frac{3}{8}$ inch in height and $\frac{1}{2}$ an inch in diameter. Its lower extremity is a little convex and immersed in the mercury; it is also easily moved vertically by means of the handle or stem G L, which passes through a cap covering the mouth of the tube E F, and having a hole, l, in its centre, wide enough to receive the handle. This part of the piston is also attached to the frame A a by a loop or socket of brass, L, in which it would move vertically with perfect freedom, were it not for the pressure of a bent spring which is situate between L and l, and acts upon the handle G l, with a force, that keeps the piston in

Use of the piston.

any position assigned to it by the operator. The use of this apparatus is almost too obvious to stand in need of an explanation; its office is to place the surface of the mercury in the leg D E F on a level with O o, when an observation is to be taken with the instrument. This is done by placing the finger upon G, and pressing the piston downwards, when the barometer is rising, which operation must be continued until the quicksilver coincides with O o. On the contrary, when the mercury falls in the leg B C, the piston must be drawn up by means of the handle G L, so as to produce a coincidence in the opposite branch of the tube, similar to that just now mentioned. One circumstance must be attended to in the construction of the instrument; for it is necessary that the middle of the piston K, which is marked with a dotted line in the figure, should coincide with O o, as oft as the barometer stands at 29·5 inches. The method of providing for this coincidence will be easily pointed out by an example: suppose, when the tube is filled, that the middle of the piston and the surface of the mercury in the leg D E F coincide with O o, while the height of the barometer is 29·00 or $\frac{1}{2}$ an inch less than 29·5; in this case, raise the piston until the mercury in B C stands at 28·5, or as much below 29·0 as 29·0 is below 29·5: this being done, pour mercury into the leg D E F, so as to make the opposite column rise again to 29·0, and the required coincidence will be provided for. On the other hand, suppose the instrument to be adjusted as above described, and the height of the barometer to be 30·0, or $\frac{1}{2}$ an inch above 29·5: in this case push the piston downwards, until the column in B C stands at 30·5, or as much above 30·0 as 30·0 is above 29·5: this being done, take a quantity of mercury out of the leg D E F just sufficient to make the opposite column fall again to 30·0, and you will have secured the coincidence required.

A necessary precaution in the construction of the instrument.

Remarks respecting the dimensions of the piston.

I have observed in a preceding part of this letter, that the piston of my barometer is $1\frac{3}{5}$ inch in length, and $\frac{1}{2}$ an inch in diameter; but the reader is not to imagine, that these dimensions are fixed by necessity, for they may be varied at pleasure; and the following rule will determine the one, when the other is given. 1st. When the diameter of the piston

piston is given, multiply the length of the scale by the square of the internal diameter of the leg B C, and divide the product by the square of the given diameter, and the quotient will be the length of the piston: 2d. When the length is given, divide the product found above, by the given length, and the square root of the quotient will be the diameter of the piston. In adjusting these dimensions, we have two circumstances of some importance to attend to; the quantity of mercury requisite to charge the tube increases with the length of the piston, which suggests a consideration of an economical nature; but if the diameter of the piston be too much augmented, to avoid expense, the free motion of the barometer will be considerably impaired; the artist must therefore use his own judgment in giving those dimensions to the piston, which will be the most convenient for the tube he is going to fit up.

I remain, &c.

JOHN GOUGH.

II.

Observations on the Phytolacca, or American Pokeweed; by Mr. H. BRACONNOT, *Member of the Academy of Sciences, &c. at Nancy*[*].

MAN, who lays all nature under contribution, to increase his enjoyments, has availed himself of a great number of vegetable productions; but, notwithstanding his extensive researches, he is yet far from being acquainted with the properties that characterise the majority of plants. The phytolacca, which has been greatly neglected, may furnish an instance of this. The properties of many plants remain to be examined.

SECT. I.

Incineration of the phytolacca.

THIS plant, which is acrid, has a very thick, fleshy root, as big as a man's leg. Its stalks are as big as a large walking stick, six or seven feet high, and purple. Phytolacca.

[*] Abridged from the Annales de Chimie, Vol. LXXII, p. 71, April 1807.

If

Stalk yields potash. If a piece of the stalk be exposed to the flame of a candle, it is reduced to a reticular texture, exhibiting, when viewed by a lens, a series of longitudinal filaments connected by cross meshes. If this be again exposed to the flame, it swells up, melts, and the result is potash.

14 oz. troy incinerated. Four woody stalks of this plant weighed when dry 440 gram. [14 oz. troy]. These I burned in an iron crucible; and when it began to grow red hot, the matter assumed a pasty consistence, and ended by fusing, accompanied with a swelling up occasioned by the evolution of hidrogen gas, which burnt with detonation as it burst from the melted matter. When the crucible was cold, it contained a hard brown substance, that had a caustic taste.

Coal lixiviated. As it was impossible to get this saline residuum completely out of the crucible, I boiled water in it, and thus easily separated it, great part of it being dissolved. The liquor

432 grs. of salt. filtered and evaporated to dryness left 28 gram. [432 gr.] of a saline substance, which I saturated with pure nitric acid. The liquor deposited a blue precipitate, which weighed 4 decig. [6 gr.]. This precipitate was not altered by muriatic acid, and appeared to me to be prussian blue with a

A little silex. little silex.

In the solution saturated with nitric acid pure nitrate of strontian occasioned no precipitate: but nitrate of silver threw down some muriate of silver, which weighed when

8·5 grs. of muriatic acid. dry 22 dec. [34 gr.[. answering to 25 cent. [8½ grs.] of muriatic acid.

After having separated a little silver from the liquor by means of sulphuretted hidrogen, I filtered, and evaporated to dryness. Thus I obtained 33 gram. [510 grs.] of nitrate

270 grs. of potash. of potash; containing 17·5 gram. [270 grs.] of pure potash according to the analysis of Thenard. These 33 gram. of potash contained no foreign matter, for they crystallized to the last particle.

6 grs. of silex. The part insoluble in water being treated with nitric acid, 4 decig. [6 gr.] of silex were left. Carbonate of potash

110 grs. of lime. threw down from the nitric solution 13 gram. [200 grs.] of carbonate of lime; and the filtered liquor, being boiled, let

A little magnesia and lime. fall a few decigr. of carbonate of magnesia and lime. It is probable, however, that the lime, which constitutes the greater

greater part of the insoluble portion of the ashes, is not in the state of carbonate in the plant, but saturated, as well as the potash, with another acid, which will be mentioned presently.

From these observations we may infer, that a hundred pounds of the ashes produced by incinerating the phytolacca will yield 66 lbs. 10 oz. 5 dr. of dried alkaline carbonate nearly pure, and containing about 42 lbs. of pure and caustic potash*.

100 lbs. of ashes give 66 lbs, 10⅝ oz. of carbonate of potash.

Sect. II.

Examination of the acid, that neutralizes the potash in the phytolacca.

I BOILED 4½ hectogr. [14½ oz. troy] of the fresh woody stalks of this plant in a quantity of water. The decoction did not change infusion of litmus. On evaporating to a sirupy consistence, 1 gram. [15½ grs.] of a salt confusedly crystallized was deposited by standing some time. Of this the greater part was soluble in water, 2·5 decig. [3¾ grs.] of a white powder remaining, which were dissolved in nitric acid. With this solution nitrate of lead gave a white precipitate, which, dried and put on a red hot iron, gave a little smoke, and left a yellow oxide of lead, soluble in a weak acid.

14½ oz. of the stalks boiled yielded 15½ grs of neutral salt.

The soluble part of the saline deposit did not afford any very distinct crystals on being evaporated, but a saline crust of a very pungent taste was formed.

Not distinctly crystallizable.

This salt swells up and is carbonized when exposed to the fire, and leaves as a residuum carbonate of potash. The same salt, when heated with concentrated sulphuric acid, is blackened, and produces sulphurous acid.

Swells up, burns, and leaves carbonate of potash.

Lime water, and the nitrates of lime, strontian, and lead, form white precipitates in its solution, which are insoluble in distilled vinegar.

With lime, strontian, and lead, insoluble in vinegar.

* As potash diminishes considerably in vegetables in proportion as they approach the woody state, it is probable, that the phytolacca would afford a much greater proportion of potash in an earlier stage of its growth.

These

The decoction diluted and precipitated by nitrate of lead.

These experiments announced a deliquescent and difficultly crystallizable salt in the phytolacca: and to separate its elements, I diluted the decoction, which had been evaporated to a sirupy consistence, with a sufficient quantity of water. To this I added a solution of nitrate of lead, which formed a very copious precipitate; and the filtered liquor afforded an abundant crystallization of nitrate of potash.

With sulphuric acid gave the smell of uree.

The precipitate, after being well washed and dried, weighed 5 gram. [77 grs.]. Being treated with a fourth of its weight of sulphuric acid diluted with six parts of water, a tolerably decided smell of uree exhaled from the mixture, which had been exposed to a gentle heat on a sand bath; and the liquor was filtered. What passed through was acid; did not crystallize by evaporation or standing; and on continuing the evaporation a glutinous matter remained, yellowish, attracting moisture from the air, and carbonized by a stronger heat.

The acid liquor.

Farther examined.

In this acid the nitrate of lead produced a very copious flocculent precipitate; and the precipitate, exposed to the blowpipe, was immediately reduced to a globule of metallic lead.

The nitrates of lime and strontian produced no perceptible alteration in it, but ammonia occasioned a precipitate.

It precipitated lime water; and the precipitate put on a hot iron began by carbonizing, and left some ashes, which dissolved with effervescence in nitric acid.

What remained of the acid I saturated with soda, but no crystals were produced by evaporation. The result of this combination, when heated in a crucible, burned, leaving a light, alkaline coal, that effervesced with nitric acid.

Analogous to the malic.

Points in which they differ.

From the properties here mentioned, it appears, that the acid of phytolacca has considerable affinity to the malic, but with a few shades of difference. With lime and lead malic acid forms flocculent precipitates very easily soluble in distilled vinegar; but those with the phytolaccic acid are insoluble.

Perhaps intermediate between the malic and oxalic.

This acid may probably be a mean between the malic and oxalic, or an oxigenized malic acid; but as it is very abundant in the phytolacca, it will be easy to ascertain this by farther examination of its saline combinations.

On

On the contrary, should it prove to be malic acid, it would be at least the first example of malate of potash found abundantly as a natural production

Sect. III.

Examination of the colouring matter contained in the berries of the phytolacca.

The colouring matter.

The berries being bruised in a glass mortar with a certain quantity of water, the filtered liquor was of a fine bright purple colour. I at first attempted to fix it on cloths, but soon found its extreme fugaciousness.

The juice of the berries.

The juice of the berries has a sweetish taste, leaving behind a sensation of acrimony. Paper tinged blue with litmus, and wetted with water, was reddened when dipped into it; but the blue was restored without the least alteration by washing with a little warm water.

Soon ferments. Taste bad, but yields spirit.

At a moderate temperature it soon underwent the vinous fermentation. The wine produced was unpleasant to the taste, but brandy may be obtained from it by distillation.

Lime water turns it yellow.

If a few drops of lime water be added to the juice, it soon assumes a fine yellow colour; but the smallest quantity of acid soon restores its purple hue. If the yellow liquor have sufficient colour to write with it, breathing on the paper will change the yellow letters to purple; and so will even exposure to the air, though less speedily.

Any acid,

sulphuretted hidrogen, or urine, restores the purple.

Sulphuretted hidrogen, or urine added in small quantity to the yellow liquor, changes it immediately purple.

Water dilutes the yellow, and in large quantity restores the purple.

The deep yellow liquor produced by the combination of the purple of phytolacca with lime grows lighter by the addition of water, and assumes the tint of chromate of potash. But if the quantity of water be still increased, the original purple reappears. At first I ascribed this effect to the carbonic acid, that might be contained in the water; but water long boiled exhibits the same phenomenon. Hence I infer, that water acts by weakening the effect of the lime in the yellow liquor, which occasions it to return to purple.

A delicate test of acids:

From what has been said it appears, that the yellow liquid affords a very delicate test of the presence of acids; and

and a comparative experiment with litmus will corroborate this. Into two glasses I put equal quantities of the juice of phytolacca, which I had turned yellow by a few drops of lime water: and into the two other glasses I put an equal weight of infusion of litmus, of an equal depth of colour. More than 60 drops of a very weak acid were required to redden the infusion of litmus, but less than 15 restored the purple colour of the yellow liquor. Hence it follows, that the yellow liquor of phytolacca is at least four times as sensible as the infusion of litmus; but the yellow liquor being extremely fugacious, it cannot be kept, or even used but just after it is prepared. A few hours are sufficient to change it.

but does not keep.

Its spontaneous changes.

First a fallow precipitate falls down, which, looked at in the sun, exhibits very small scales with the various hues of the opal. After a few days brown flocks separate from it. The properties of the reagent are then entirely destroyed, and at length the liquor is almost wholly deprived of colour.

The following are the results produced in the purple liquor by other reagents.

Effects of reagents on the purple liquor.

Caustic alkalis give it a yellow colour. Alkaline subcarbonates, a fine violet, that fades, and becomes yellow, by standing. Weak acids, nothing perceptible. Dilute oxigenized muriatic acid, a complete deprivation of colour, with white flocks. Alum, nothing apparent on mixing, but after some days a very light red precipitate. Muriate of lime, no change. Muriate of tin, a red sediment inclining to lilac, and leaving the supernatant fluid colourless. Nitrate of lead, a precipitate of the colour of wine-lees. Superoxided sulphate of iron, a dirty violet: and on adding an alkali a very deep green precipitate, changing yellow by exposure to the air.

The purple colour that tinges the epidermis of the stalks of the phytolacca is precisely of the same nature as that contained in the berries, and afforded the same results.

The colouring matter of the stalks the same.

I have convinced myself, that this purple does not arise from the alteration of any colour by an acid: for having changed it yellow by ammonia perfectly freed from carbonic acid, impregnated linen with this, and exposed the linen to a moderate temperature excluding the air, the purple reappeared in all its lustre as the ammonia was volatilized.

Not a colour altered by an acid.

This

This colour is different therefore from that of some other fruits, as the plum and cherry, which becomes green on the addition of alkalis; and from that of litmus, which alkalis turn blue: but it appears to have some analogy with that of the grape, as lime water turns red wine of a dirty yellow, which acids change again to red.

Differs from the red juices of some other fruits.

Analogous to that of the grape.

Sect. IV.

Other properties, and cultivation of the phytolacca.

In North America the leaves are boiled, and eaten as spinach, and I have found them very good*. The juice of the root is a purgative, and may be taken in the dose of a table-spoonful or two; but must not be used when there is any inflammation of the viscera. The narcotic virtues, that have been ascribed to it are illusory, as Lemery observed.

Its leaves esculent.

Juice of the root purgative.

It may be propagated by seed, sown in the spring in light ground, and transplanted to a dry soil, which should be dug very deep. When the plants have taken root, they require no care, but to be kept free from weeds. The stalks die with the first frosts, but the roots are perennial, and throw out shoots in the spring for several years.

Culture.

III.

A Memoir on the Proteus Anguinus; by Baron von Zoïs†.

THE proteus anguinus is found in Carniola, between Sittich, an ancient monastery about eight leagues from Laybach, on the road to Neustadt, and a small village called Vir in the Sclavonian language, and Weyer in German.

Where found.

* The young shoots are said to be as good as asparagus. Tr.

† Translated from the Italian manuscript of Baron Zois by Mr. Siauve, commissary at war; and inserted in the Magazin Encyclopédique for January, 1807, p. 39; whence this article is taken.

The

Geology of the place.

The rock that composes the hills of Sittich is a compact, stratified, calcareous mass, rising in the centre of our Alps to the height of 1500 toises above the level of the Adriatic; and the geological character of which is to be interspersed with funnel-shaped hollows on the surface, and grottoes and caverns internally.

At the foot of a part of this calcareous rock, at the bottom of the valley of Vir, are two openings or mouths of grottoes, 15 or 18 inches in diameter, 3 or 4 feet above the surface of the ground, and about 12 feet distant from each other. From each of these a stream of cold and limpid water flows into a small basin beneath, which is afterward lost in the ground about 750 paces beyond the village. The amphibious animal in question has never yet been found in Carniola, except in these two basins: and as it is never seen in them, except on the melting of the snow, or after heavy rains, it is supposed, that the overflowing of the subterraneous reservoirs, to which they belong, drives them out. The peasants of Vir, who know them by tradition as well as experience, call them *bela riba*, white fish, or *zhloveshka riba*, fish that has something human. The latter name alludes to the joints of their toes, or fingers, and the colour of their skin.

Seen only in two basins, supplied from subterraneous caverns, and after inundations.

Popular names.

By whom noticed.

The animal was first made known to the public by Dr. Laurenti, in his *Synopsis Reptilium*, in 1768. He gave it the name of proteus anguinus. Scopoli, who saw it alive, gave a fuller description of it in 1772, in his *Annus Quintus Historico-Naturalis*. He says, that Linneus, to whom he sent it, considered it as the larva of a lizard; but he thought it a distinct genus. Linneus however expressed himself doubtingly both on this and the proteus tritonius of Schranck, found in lakes in the interior parts of Austria; leaving it to accurate and repeated observation to decide, whether or not they underwent a transformation at a late period.

Linneus suspected it to be a larva.

Dissected.

To forward the solution of the problem, Baron Zois sent several specimens of the proteus anguinus preserved in spirit to Dr. Schreiber, professor of natural history at Vienna, that he might dissect them. The anatomical description is given at length in the Philosophical Transactions for 1801.

Its analogy to larvæ of some

The principal analogy between the proteus and the larvæ of some amphibia, which has occasioned them to be confounded

founded*, consists in the gills common to both. Mr. Schreiber however observes, that the gills of the proteus differ essentially from those of larvæ and fishes by their red colour, owing to the blood which it causes to circulate through them more or less abundantly at pleasure.

Amphibia in the gills. Difference.

As to the organs of respiration, Mr. Schreiber asserts, that, having dissected a great number of larvæ of aquatic lizards, he never found the least analogy between them and the proteus. He considers it as more allied to the siren lacertina, both having gills and lungs; though Camper indeed denies the existence of lungs in the siren. It is true Mr. Schreiber observes, the siren differs in having but two feet; a short head without any beak; a small, pointed mouth; eyes very apparent, and eyebrows; and the lungs, though equally formed of one single membrane, without any cellular divisions, running along both sides of the body, and exhibiting neither the various turns nor the very remarkable bladders found in those of the proteus.

More analogous to the siren lacertina. Differences. Lungs.

That Mr. Schreiber found it difficult to discover and ascertain all the parts of the organization of specimens, that had been kept a long time in spirits, is not at all surprising. He observed ovaries however, and even something that had the form of a uterus: but he lays no great stress on these slight appearances.

Difficult to ascertain its parts. Apparently ovaries and a uterus.

I hope however he will be able to decide the question, by means of the dissection of some individuals, which I have found means to send him alive. The basins at Vir had furnished only three, four, or five in a year since 1798: but on the 26th of December, 1804, on the thawing of a deep snow, fourteen were brought me at once. These are at present at Vienna, some of them under the eye of Mr. Schreiber, and others in a subterraneous canal, under circumstances the

Only from 3 to 5 found in a year. Once 14.

* The question appears at present decided, and the protei are considered as a distinct genus Beside the anguinus, which had already been figured and described, but of the manners and habits of which Baron Zois gives here new and interesting particulars, and the tritonius of Schranck, Humboldt and Bonpland observed another species in Peru, the skin and limbs of which perfectly resemble those of the salamander.

most

most favourable for their nourishment, and for their breeding, if they be capable of it*.

Manners. As to the manner of living of the proteus, whether it be a perfect animal or not, its principal character is a very decided

Strong antipathy to light. antipathy to daylight. Exposed to the sun it is agitated in an extraordinary manner, and makes continual efforts to escape.

No external eyes: Yet it has no eyes externally, or, as Scopoli asserts, two tubercles in the place of eyes. Mr. Schreiber first dis-

but two underneath the skin. covered its small, black, subcutaneous eyes, which are perceptible sometimes, though but rarely; and this only in individuals that have grown lean by forced abstinence, and the epidermis of which is become very thin.

Moves by means of its paws, or tail, slowly. In its movements under water the proteus sometimes employs its paws, or feet; at other times its tail, in different manners. Its progress is slow and circumspect: but when it

When offended quickly and like an eel. is irritated it flees with swiftness, and with a sinuous motion like an eel. In this case it makes no use of its feet; and as those behind might impede its velocity, it keeps them close to its body.

Conceals itself in the day: at night in motion. During the day it likes to keep itself concealed, and seldom changes its place: by night on the contrary it is always seen moving about at the bottom of the water, and frequently attempts to get out.

Takes no food in confinement. Those that are thus in captivity would never touch any of the food offered them, such as fresh eggs of fishes, fibres of fishes or frogs, aquatic worms, polypi, conferva, &c.; not even the helix thermalis, though it is certain in a state of liberty they swallow a great number of these testaceous animals; for I have found as many as eighty-four of their shells in the excrements of a well fed proteus, which he discharged at three times the second day of his captivity. When living ones were given him, he took up one with his

Receptacle formed for them by the archduke. * The archduke John has had a subterranean canal constructed of tufa. A stream of spring water runs through the sinuosities formed in it, and fills the different basins, in which the protei are. A spring was selected, the waters of which contain insects adapted for their food; and means have been contrived for inspecting the little colony, when it may be thought time. It is to be wished by the epicure, that the protei may breed, for their flesh is white, delicate, and of an exquisite flavour, as I have been assured by Baron Zois, who has eaten it. *Note of Mr. Sieuve.*

mouth

mouth, but immediately threw it out to the distance of two inches. Afterward he chose rather to leave them to breed, than to taste them.

Lives a long while in water

Notwithstanding this obstinacy in refusing all kind of food, these animals live a long time in pure water, if they be kept in the shade in wooden tubs 15 or 18 inches high, and as much in diameter; and if the temperature of the water, which ought to be changed every four or five days, do not exceed 8° of Reaumur [50° F.]. Put into cold water in which snow was floating, I have seen them experience painful sensations, manifested by their agitation, and die within four and twenty hours.

about 50° F.

Know one another.

When put together they appear to know one another, for the new comers soon grow familiar with the old ones, and sometimes even utter a gentle cry, that seems to indicate their satisfaction*: on the contrary, when they are offended by the curiosity of the spectators, or the appearance of the sun, they hiss with a harsh tone, very different from the preceding, and at the same time emit bubbles of air from the mouth and gills. The degree of their agitation may be distinguished by the greater or less tumefaction of the branchy plumes that rise from the gills, as well as from their colour, which in this state assumes a fine crimson. If these plumes be then viewed with a microscope, they resemble branches of coral; but when the animal is tranquil they fall, become flaccid, and are of a livid white.

Utter a cry of satisfaction, and a hiss of displeasure.

Express offence by their gills.

Circulation of red blood seen in the gills.

With a lens of moderate power the systole and diastole of the pulse may be perceived in these plumes, the branches

* The proteus appears to possess the sense of feeling in an eminent degree. I have often been pleased to see it pass its little hands over others of its own species.

However this may be, the resemblance of its fore paws to the human hand, the fine carnation tint of this pretty animal, the transparent epidermis that may be compared with that which covers the person of a handsome woman, the sort of amorous cry it utters when it has a companion given it, have thrown me more than once into a revery. I know not whether the proteus possess more than ordinary intelligence: but every thing, even to its obstinacy in refusing all kind of nourishment, interest me in favour of this *fish-man*, as the Carniolians call it. *Note of Mr. Sieuwt.*

of

of which are so many tubes; and the red globules of blood may be seen to ascend and descend at each pulsation. I have counted from forty-five to forty-eight of these pulsations in a minute.

Out of the water soon sticks to the ground and dies.

Out of the water the proteus cannot proceed more than a few feet, for the glutinous substance, with which it is enveloped, soon dries, and glues it to the ground by the part on which it drags itself along. Several of mine died in this state. Having found one thus, that had still some signs of life, I separated it with my hand; but it died two days after, having a red streak, indicating inflammation, along the side which had adhered to the ground. Others after it, which were separated by means of warm water, continued to live. In all that died under my inspection I observed an infallible sign of approaching death: the gluten, with which they are covered, begins to separate from the body, and becomes visible in the water; it floats about them in flocks, attaching itself particularly to the paws and tail; at length they turn on their backs, and soon come to die on the surface of the water.

Before death their slimy coat separates.

Size from 7 in. to 15, by 6 or 18 lines.

The annexed figure of a proteus anguinus drawn from nature, Pl. III, fig. 2, gives an accurate idea of its external form. With respect to size, they have been found from seven or eight to twelve or thirteen inches long, and even fifteen or sixteen; and in diameter from six to eighteen lines. These dimensions do not agree with those of the aquatic salamander, and there is no other reptile in Carniola, that can be compared with it.

Three have been kept 28 months.

I have at present three, that have lived in pure water two years and four months. They have grown thin, and diminished in size one half; and the fin that forms the tail even appears to be shortened: yet they are in perfect health. I shall not fail to observe the duration of their lives; though I begin to persuade myself that the vague term, which Linneus expresses by the words transformation at a late period, is already past.

IV.

Account of the Antique Vessel, that was preserved at Genoa under the name of SACRO CATINO, *and reputed to be an Emerald; with the Report made of it to the French Institute, August* 4, 1806, *by Mr.* GUYTON*.

A Dish of an hexagonal shape had long been in possession of the city of Genoa, which was supposed to be an emerald, and consequently of inestimable price. Farther to enhance its value, according to the legend it was the very dish, on which the paschal lamb was served up, when Christ celebrated the passover with his apostles, afterward miraculously converted into an emerald; though some will have it to have been originally an emerald, and among the presents of the queen of Sheba to Solomon. Neither is it agreed in what manner it came into the possession of the Genoese; as some say it was their part of the booty found at the taking of Cæsarea in the first croisade; others, that it was presented to them by Baldwin king of Constantinople. It was so highly valued, however, that at the siege of Genoa in 1319, it was mortgaged for 1200 marks of gold, and redeemed eleven years after.

Sacro Catino.

Legend respecting it.

Brought from the East.

Mortgaged for 1200 marks of gold.

An act passed in 1476 to prohibit its being touched with gold, silver, gems, coral, or any other substance, under very heavy penalties, and even in some cases pain of death, shows, that suspicions of its genuineness existed. William of Tyre is the first we know, that expresses such a suspicion. Barthelemy observed in it blebs, which induced him to think it glass. Condamine too, examining it by torchlight, and at some distance, could not perceive in it any of those clouds or defects of transparency, that are common in emeralds as well as in all gems of a certain size; yet he evidently discerned several small vacuities, resembling air blebs, both round and oval.

Suspicions of its being glass.

* Abstracted from Mag. Encyclopédique, January, 1807, p. 137; and Annales de Chimie, Vol. LXI, p. 250, March, 1807.

Removed to Paris, and examined.

At length this vessel having been transferred to the imperial library, the Institute was requested by the minister to ascertain its quality; and Haüy, Vauquelin, and Guyton, were accordingly appointed to examine it. What follows is the substance of Mr. Guyton's report.

Its figure.

Its diameter, from one angle to another, is 39·143 cent. [15 in. 2 l. Eng.], its depth within 12·357 [4 in. 8 l.], its height, including the foot, which is of the same piece, 16·476 [6 in. 4 l.]. It has two handles, likewise formed in the same piece, without any appearance of having being joined to it after it was made. One of these is broken. The bottom appears to have been wrought on the wheel, so as to form a circle of small cavities, whence issue six rays, corresponding to the angles. Pl. III, fig. 3, is an exact representation of the bottom; fig. 4 shows it standing on its foot, and fig. 5 inverted, with the position of its handles, which are placed so as to be concealed, yet in a manner to be taken hold of easily.

Colour.

The colour of the dish is an olive green, duller than that of the peridot, with something of a greasy cast, that brings it nearer to the plasma of the German mineralogists, than to the green emerald of Peru, or the bluish emerald, or aqua marina, of Siberia. Its transparency participates of this tint.

Has blebs.

On holding it up against the light, a bleb about 2 millim. [·78 of a line] is distinguishable near the centre, and farther off some very small blebs.

These marks doubtful.

It would be difficult from this description to infer the nature of its substance, since it is now well known, that the colour of gems of the same species varies considerably, and that rock crystals exhibit blebs interiorly, which we cannot always distinguish with certainty from those in glass.

Spec. grav. not examined.

We could not conveniently examine its specific gravity, on account of its size: and besides it would have been to little purpose, as the specific gravity of the emerald does not much exceed that of common glass, and is inferior to that of a glass loaded with metallic oxides.

Hardness.

We ascertained its hardness, however, which is a less equivocal character of gems; and found it very evidently scratched not only by the emeralds of Peru and Siberia, but

but even by rock crystal. This would be sufficient to decide, that it could not be a real emerald, if the enormous block, from which such a vessel must have been cut, would not be a phenomenon, that nothing hitherto found in nature renders probable. Not an emerald.

It is a manifest errour in Dutens, to admit among the varieties of the Peruvian emerald a stone that can be touched by the file. Those of the environs of Limoges, which are scarcely transparent, scratch flint. Mistake of Dutens.

The largest emeralds known, before the discovery of the colourless emeralds in the department of Haute-Vienne, and mentioned in the inventory of the Garde-Meuble, the catalogues of Davila, Daugny, &c., and those that Condamine saw at Rome, which he considered as enormous, did not exceed 10 or 12 cent. [3 in. 9 l. or 4 in. 7 l.] long, by 3 or 4 [1 in. 2 l. or 1 in. 6 l.] broad. Even those of the commune of Bessines have yet afforded only masses of 30 or 40 cent. [11 in. 7 l. or 15 in. 6 l.] high, by 15 or 16 [5 in. 8 l. or 6 in. 2 l.] thick. Largest emeralds known. Those of France.

We may judge what would be the value of the dish, if it were a real emerald, by taking the rule of its being worth one fourth the price of a diamond of equal weight.

What has been said is sufficient to authorise us to conclude, that the substance of the dish cannot be considered as an emerald, or any other gem, but is coloured glass. We leave to others to determine its value, either as a work of art, or as an antique; and whether it may be identified or compared with that which Herodotus says he saw at Tyre, in the temple of Hercules. We shall only observe, that the art of imitating gems by coloured glass dates from remote antiquity. Pliny speaks in several places of these false stones, and points out the method of distinguishing them, by trying their hardness against others. As it is coloured glass it is valuable only as an antique.

V.

On the Cultivation of the Crambe Maritima of Linnæus, or Sea Kale. By Mr. JOHN MAHER, *F. H. S.*[*]

Culture of sea kale

IF the man who makes two blades of grass grow where only one grew before, is to be esteemed an important benefactor to his country; he who teaches us how to improve a palatable and nutritious vegetable, hitherto often neglected, upon the barren cliffs of our sea-girt isle, has surely no small claims to our gratitude: as such, I must ever regard those

improved by Mr. Curtis.

of the late Mr. Curtis, from whose pamphlet upon the *Crambe Maritima*, or *Sea Kale*, I first learnt how to grow this early esculent; but as his useful directions are yet in the hands of comparatively few of my brother gardeners,

Very fine produced at Edmonton.

and as the young shoots have been obtained at Edmonton of a size and delicacy greatly superior to what generally appears at the table, I venture to offer a particular account of the method of cultivating it there to the Horticultural Society.

Places where found wild.

The particular places on record where this plant grows wild, are below *Maryport;* also between *Ravenglass* and *Bootle*, in Cumberland; at *Roosebeck*, in Low Furness, Lancashire; near *Conway*, plentifully, but in the most inaccessible rocks; promontory of *Llyn*, and near *Cruccaeth*, in Caernarvonshire; between *Rhuddlgaer* and *Llandwyn*, in the isle of Anglesea; about *Port Inon*, in Glamorganshire; near *Megavissey*, in Cornwall; marly cliffs, near *Teignmouth*, and *Sidmouth*, in Devonshire; on *Chesil Bank*, chalk cliffs at *Weymouth*, *Lulworth Cove*, and about *Poole*, in Dorsetshire; at *Western Court*, in Hampshire; near *Worthing* and *Shoreham*, cliffs at *Beachy Head*, and near *Hastings*, in Sussex; between *Folkstone* and *Dover*, at *St. Margaret's* and *Langdon Bays*, between *Whitstable* and the *Isle of Thanet*, at Lidde, in Kent; near *Harwich*, in Essex; on the north coast of *Norfolk*, abundantly; near *Fast-castle*,

[*] From the Transactions of the Horticultural Society of London, Vol. I, Part I, p. 18. For an account of the objects of this society see Journal, Vol. XIV, p. 150.

Berwickshire.

Berwickshire. According to Dr. Smith, sandy shores are its natural soil, but by what I can learn from others, as well as my own personal observation, it prefers loamy cliffs, mixed with gravel. I found it near *Dover*, also in *Sussex*, in stiff loam: to the extensive beach of pure sand, both above and below *Scarborough*, in Yorkshire, it is, I believe, quite a stranger. Natural soil.

The whole plant is smooth, of a beautiful glaucous hue, covered with a very fine meal; occasionally, however, it varies like the wall-flower-leaved ten weeks stock, with quite green leaves. Root dark brown, perennial, running deep into the ground, divided into numerous wide spreading branches, but not creeping*. Radical leaves very large, and spreading wide upon the ground, waved, more or less sinuated, and indented, containing a bud, or rudiment of the next year's stem at the bottom of the leafstalk, dying away in the autumn†. Stems several, from one foot and a half to two feet high, erect, branching alternately, and terminating in large panicles of spiked flowers, which smell somewhat like honey. Peduncles, as the fruit swells, considerably elongated. Calyx often tinged with purple, its leaflets nearly equal. Petals cream coloured, with purple claws, larger than in many genera of this natural order. Filaments purple. Anthers pale yellow. Glands of the receptacle between the longer filaments yellowish green. Stigma pale yellow. Pouch, as the accurate Mr. Woodward describes it in Withering's work, at first egg-shaped, afterwards nearly globular, fleshy, falling off when ripe, about August, with the seed in it, which is large, and of a pale brown colour. Description.

* Root not creeping, in the proper sense of that word, as Parkinson, Miller, and Bryant have described it; but if the branches be divided into a number of pieces, each piece will grow if committed to the earth; and as it is impossible to dig among the widely extended roots of these plants without cutting many of them, and leaving a number of fragments, plants arise from such around the original, and give to it the appearance of having creeping roots. CURT.

† Parkinson perhaps never committed a more egregious blunder, than in the account he has given of this part of the plant's economy; "*The root is somewhat great, keeping the green leaves all the winter.*" Bryant, in his *Fl. Diæt* misled, perhaps, by this account, says, "*The radical leaves being green all the winter*, are cut by the inhabitants where the plant grows, and boiled as cabbage. CURT.

The

Exported 200 years ago.

The *Crambe Maritima* was known, and sent from this kingdom to the continent more than two hundred years ago, by l'Obel, and Turner*; but our immortal countryman,

Miller noticed it.

Philip Miller, has the honour of being the first who wrote upon it professionally, as an esculent, telling us, in the first edition of his Gardener's Dictionary, published in 1731, that the inhabitants of Sussex gather the wild plant to eat in spring, soon after the heads are thrust out of the ground, otherwise it will be tough and rank. Professor Martyn, next, in the last edition of the same work, has printed some valuable additional instructions, how to cultivate this plant, from the MS. of the Rev. Mr. Laurent. Lastly, the late celebrated Mr. Curtis has done more to recommend it, and diffuse the knowledge of it, in the dissertation above quoted, than any of his predecessors.

Mode of culture.

To grow this vegetable in the highest perfection, prepare the ground in December or January, by trenching it two feet and a half deep; if not that depth naturally, and light, it must be made so artificially, by adding a due proportion of fine white sand, and very rotten vegetable mould. If your ground is wet in winter, it must be effectually drained, so that no water may stand within a foot at least of the bottom: for the strength of your plants depends on the dryness of the bottom, and richness of your soil. Then divide the ground into beds, four feet wide, with alleys of eighteen inches, after which, at the distance of every two feet each way, sow five or six seeds two inches deep, in a circle of about four inches diameter; this operation must be performed with strict care and regularity, as the plants are afterwards to be covered with the blanching pots†, of which a drawing

* It would be difficult to ascertain the precise period of its being first used with us as a culinary plant; on many parts of the seacoast, the inhabitants for time immemorial have been in the practice of seeking for the plant in the spring, where it grows spontaneously; and, removing the sand or pebbles, they cut off the young shoots as yet blanched, close to the root. Mr William Jones, of Chelsea, saw bundles of it in a cultivated state, exposed for sale, in Chichester market, in the year 1753. Curt.

† It appears to me, that for forcing, it would be a great improvement to make the blanching pots in two pieces, the uppermost of which should

a drawing is annexed, Pl. III, fig. 6, and both the health and beauty of the crop depends upon their standing at equal distances. In the months of May and June, if the seeds are sound, the young plants will appear. When they have made three or four leaves, take away all but three of the best plants from each circle, planting out those you pull up (which by a careful hand may be drawn with all their tap root) in a spare bed for extraforcing, or to repair accidents. The turnip fly and wire worm are great enemies to the whole class of tetradynamia plants. I know no remedy for the latter, but picking them out of the ground by hand; the former may be prevented from doing much damage, by a circle of quick lime strewed round the young plants. If the months of June and July prove dry, water the whole beds plentifu'ly. In the following November, as soon as the leaves are decayed, clear them away, and cover the beds an inch thick with fresh light earth and sand, that has laid in a heap and been turned over at least three times the preceding summer; this, and indeed all composts, should be kept scrupulously free from weeds, many of which nourish insects, and the compost is too often filled with their eggs and grubs. Upon this dressing of sandy loam, throw about six inches in depth of light stable litter, which finishes every thing to be done the first year.

Wire worm and turnip fly.

In the spring of the second year, when the plants are beginning to push, rake off the stable litter, digging a little of the most rotten into the alleys, and add another inch in depth of fresh loam and sand. Abstain from cutting this year, though some of the plants will probably rise very strong, treating the beds the succeeding winter exactly as before.

2d year.

The third season, a little before the plants begin to stir, rake off the winter covering, laying on now an inch in depth of pure dry sand, or fine gravel. Then cover each parcel with one of the blanching pots, pressing it very nrmly into the ground, so as to exclude all light and air; for the colour and flavour of the *Sea Kale* is greatly injured by being ex-

3d season.

should fit like a cap upon the lower; the crop might then be examined without disturbing the hot dung. SECR.

posed

posed to either. If the beds are twenty-six feet long, and four wide, they will hold twenty-four blanching pots, with three plants under each, making seventy-two plants in a bed. Examine them from time to time, cutting the young stems, when about three inches above ground, carefully, so as not to injure any of the remaining buds below, some of which will immediately begin to swell; in this method, a succession of gatherings may be continued for the space of six weeks, after which period the plants should be uncovered, and their leaves suffered to grow, that they may acquire and return nutriment to the root for the next year's buds. The flowers, when seeds are not wanted, ought to be nipped off with the finger and thumb, as long as they appear. If a gentleman does not choose to be at the expense of the blanching pots, the beds must be covered with a larger portion of loose gravel, and mats; but the time and trouble of taking away the gravel from about the plants to cut the crop, and replacing it, is so great, that there is no real economy in it. In this way *Sea Kale* has been cut in Mr. Beale's garden, which measured ten, eleven, and even twelve inches in circumference, and upon an average each blanching pot affords a dish twice in a season.

10 to 12 inches circumference.

Forced with little expense or trouble.

No vegetable can be so easily forced as this, or with so little expense and trouble; for the dung is in the finest possible order for spring hot-beds, after the *Sea Kale* is gathered. The only thing necessary, is to be very particular in guarding against too much heat, keeping the temperature under the blanching pots as near to fifty-five degrees of Fahrenheit's thermometer as may be, but never higher than sixty. For this purpose, in November and December, according as you want your *Sea Kale*, prepare a sufficient quantity of fresh stable dung, to cover both the beds and alleys, from two to three feet high; for in the quantity to be laid on, a great deal must always be left to the good sense of the gardener, and the mildness or severity of the season. It should be closely pressed down between the blanching pots, placing heat-sticks at proper intervals, which by being examined occasionally will indicate the heat below. After the dung has remained four or five days, examine the pots. Worms often spring above the surface, and spoil the delicacy of the young

Worms.

young shoots: the best remedy against which is to cover with dry sea-coal ashes, sifted neither very small nor very large; salt also effectually destroys them, and will not injure the *Sea Kale*. The crop will be ready to gather in three weeks or a month from first applying the heat, but so much mischief ensues when this is violent, that I would advise every one to begin time enough, and force slowly, rather than quickly. It is also necessary to cut the leaves off a fortnight or three weeks before they decay, from such plants as you intend to force very early.

VI.

On Grease, and some Medicinal Compounds, of which it is the Basis: by H. A. VOGEL, *Chemical Operator in the Pharmaceutic School at Paris. Abridged by* BOUILLON-LAGRANGE*.

MR. Vogel, not having an opportunity of instituting a comparative examination of the soft fat of various animals, confined himself to hog's lard, the most common, and of most extensive use. Lard.

Lard, exposed for two months to the rays of the sun, without access of air, acquires a very pungent rancid smell, an acrid taste that affects the throat a long time, and a yellow colour, but no acidity. By the joint action of light and air the same phenomena take place, and in addition it becomes acid. Effects of light on it. Of air.

It melts at 104° or 108° F., and remains in fusion at this temperature without being decomposed; but above 212° decomposition commences. If it have been well washed, it affords no traces of ammonia on distillation. Of caloric.

Mixed with half its weight of washed flowers of sulphur, forming what is commonly called sulphur pomatum, and examined four days after, as well as when kept much longer, no trace of sulphuric acid was discoverable. By gentle fu- Dissolves sulphur.

* Abridged from the Annales de Chimie, Vol. LVIII, p. 154, May, 1806.

sion

sion on a water bath a portion of the lard was separated, and poured off; and by straining the rest through fine linen the greater part was obtained. It was of a gray colour, and a very strong, acrid, bitter taste; stood more readily on cooling; and blackened silver. If sulphuretted lard be boiled, decanted, and cooled quickly, part of the sulphur precipitates: but if it be cooled slowly it crystallizes in fine needles.

This compound distilled yields sulphurretted and carburetted hidrogen & carbonic acid:

If this mixture be distilled in a coated glass retort, to which a receiver is adapted communicating with a mercurial trough, a large quantity of gas is obtained, which appears to be a mixture of a great deal of sulphuretted hidrogen, some carburetted hidrogen, and a little carbonic acid. No sulphurous acid gas was found, as many chemists assert.

lard mixed with sulphur sublimes;

As soon as the gasses cease to come over, thick, white vapours rise, that condense with difficulty, and a yellow matter sublimes into the neck of the retort, which is lard mixed with a little sulphur. The liquor in the receiver is milky, and on cooling affords small crystals in white scales, which

a bulky coal remains.

are sulphur in a state of extreme division. A very bulky, shining, iridescent coal remains in the retort.

Sulphuretted hidrogen not dissolved in it.

Sulphuretted hidrogen gas, passed through melted lard, produces no change, and does not dissolve in it.

Dissolves a little phosphorus.

Half an ounce of lard being melted on a water bath, two grains of well purified and very transparent phosphorus were added, and kept a quarter of an hour at the same temperature; care being taken not to shake it too much, that the air might not acidify the phosphorus. When cold, some of the phosphorus was found undissolved. The lard had acquired a slight smell of garlic, and a disagreeable taste: it reddened infusion of litmus: it formed a very copious black precipitate with nitrate of silver, and a less abundant black precipitate with neutral nitrate of mercury at a minimum. An ounce of lard, brought to boil gently, was found to dissolve five grains of phosphorus; but part was precipitated by cooling. The lard was repeatedly washed with boiling water, which it rendered acrid; but it still retained some of the phosphorus in actual solution, without its being acidified.

The

The phosphuretted lard prepared with a boiling or a gentle heat, and washed or unwashed, did not shine in the dark at a temperature of 55° or 65°, even when rubbed by the hand. At 167° it was faintly luminous. Faintly luminous when heated.

Twelve grains of phosphorus being distilled with two ounces of lard, the mixture assumed a coaly aspect much sooner than lard alone. At the commencement phosphuretted hidrogen gas was evolved, which took fire in the receiver; and afterward both phosphuretted and carburetted hidrogen were obtained in a jar over mercury. The receiver contained lard, which had carried over with it phosphorus and phosphuretted hidrogen gas. After cooling, on the admission of air, it burned the lard rapidly. Lard distilled with phosphorus.

Whatever temperature be employed therefore, to dissolve phosphorus in lard, more or less phosphorous acid is always formed; whence I am induced to think, that the same thing happens in many other phosphuretted compounds. Phosphorous acid always formed.

All these experiments were made in contact with air. When air was excluded, the lard dissolved a portion of phosphorus without its being acidified; but it became acid in a few minutes, on pouring out the melted lard, or shaking it in the open air. If air were excluded, it soon becomes acid on exposure to it.

A cylinder 10 inches long and 8 lines in diameter was filled with melted lard, and immersed in mercury. Half of it being expelled in this situation by phosphuretted hidrogen gas, the cylinder was corked, removed into hot water to keep the lard in fusion, and shaken till cold. On examination it was found to have dissolved all the phosphorus contained in the gas. Takes phosphorus from phosphuretted hidrogen.

As the muriatic acid does not act on lard, and there is nothing interesting in the action of the sulphuric, Mr. Vogel confined himself to the nitric. He treated lard with it as directed by Fourcroy and Alyon for making the oxigenized pomatum. Alyon observes, that this pomatum has no need of being washed, as it is not acid. Vogel repeated his process with an ounce of acid at 32° to a pound of lard; and afterward with acid at 30°, 28°, and as weak as 24°: but he always found the oxigenized lard acid. Action of acids. Nitric always leaves it acid.

Making the experiment in a retort communicating with a pneumato-chemical apparatus, he obtained nitrogen gas, not pure, Nitrogen, nitrous, and car-

bonic acid gasses evolved.

pure, as Mr. Alyon says, but mixed with nitrous gas and carbonic acid gas, as Van Mons found.

Lard oxigenized by nitric acid.

The lard thus oxigenized is as hard as suet, and requires a heat of 113° or 117½° to melt it. Water boiled with it, and partly evaporated, acquired a lemon colour, and a rough, bitter taste; reddened litmus; and constantly precipitated acetate of lead and nitrate of mercury. Distilled in a retort almost to dryness it yields a colourless fluid containing a qnantity of acetic acid, and not precipitating the metallic solutions above mentioned.

Yields an acid to water.

The water in which it is washed, being evaporated to the consistence of a thick liquid, lets fall on cooling a brown, tenacious matter, attracting moisture from the air. The supernatant liquor being decanted and evaporated, an infinite number of small, white, very brilliant needles form in it. These Mr. Vogel took at first for oxalic acid, but lime-water was not rendered turbid by it, and it had none of the properties of oxalic acid. Its nature will be seen below.

Neither the colour nor acidity removed by washing.

Neither the yellow colour nor acidity of oxigenized lard can be removed by repeated ablutions; for after the twelfth boiling it continues yellow, and the water poured off from it reddens litmus.

Alcohol dissolves a large portion;

Alcohol comports itself differently. If it be boiled with oxigenized lard, it dissolves a very large quantity; and on cooling a great deal separates in flocks, which, collected and dried, afford an oxigenized lard strikingly whitened. The remaining lard is rendered whiter: the alcohol acquires a yellow colour, and becomes acid; and it retains matter enough in solution to form a copious precipitate with water.

which is afterward partly soluble in water.

The alcohol being evaporated, a great deal of yellow acid fat remained, which was partly soluble in water.

Does not removes its acidity.

Boiling alcohol however, employed repeatedly to wash oxigenized lard, does not deprive it completely of acidity: it rather dissolves the greater part of it, and the last liquor is still acid.

The acid separated by lime:

As the acid adheres so intimately to the lard, I attempted to separate it by salifiable bases. For this purpose I boiled it with lime water, which was thus deprived of its alkalinity, and acquired a lemon colour. This neutral liquor, which I considered as a compound of lime with an acid and lard,

was

was copiously precipitated by acetate of lead. Evaporated to the consistence of a sirup, it was divested of colour by the nitric and muriatic acids, which formed in it a whitish precipitate; and on pouring in the acid a very rancid smell was perceived.

Barytes water acts on the oxigenized lard more effectually. The orange yellow colour it acquires from it is equally destroyed by acids. I poured in a quantity of sulphuric acid sufficient to take up the barytes, boiled the whole, and filtered it at a boiling heat. The filtered liquor, which contained no barytes, was evaporated in great part on a sand heat. Small slender needles crystallized from it, interspersed with silky tufts. These were insoluble in alcohol, did not precipitate lime water, and were not sublimable in close vessels.

and more effectually by barytes.

If lard be boiled in concentrated nitric acid, and the ebullition be continued, adding water occasionally, a crystalline white powder forms in it on cooling. This powder is rough to the touch, insoluble in alcohol, and much more soluble in hot water than in cold. By its combinations with different bases, and other characters, I satisfied myself, that it was mucous acid*.

Boiling nitric acid forms pulverulent crystals, which are saccholactic acid.

Lard thus oxigenized at a maximum is soft, of a brown colour, perceptibly soluble in water, and very soluble in alcohol. The water in which it was washed being saturated by potash, the result was a foliated salt, attracting humidity from the air, and giving out acetic acid on treating it with sulphuric†. The precipitate formed by acetate of lead in this water is nothing but the lard itself, which combines with the oxide of lead, and carries down with it a little mucous acid. The former swims on the surface, when the precipitate is decomposed by sulphuric acid.

Lard oxigenized at a maximum.

Affords acetic acid.

Oxigenized lard being very soluble in alcohol, a large quantity may be precipitated from it by water. By the powerful action of concentrated nitric acid on lard a certain quantity of nitrate of ammonia is formed, as may be seen

Nitrate of ammonia formed.

* Beef suet, though it decomposes nitric acid less powerfully, likewise affords mucous acid.

† Rancid fat and very old suet likewise afforded Mr. Vogel acetic acid, when treated in the same manner.

by

pounds more or less in use, as the mercurial plaster of Vigo, ethiops saccharatus, ethiops alkalisatus, Plenck's gummy mercurial, and a number of similar mixtures. If the colour be objected, it may be observed, that antimony, however brilliant, bismuth, or any other metal capable of being powdered, becomes of a blackish gray when minutely divided.

Action on salts. Mr. Vogel next examined the action of fat on metallic salts.

Ointment of nitrated quicksilver. He prepared the unguentum citrinum by dissolving three ounces of mercury in four of nitric acid, and mixing them with two pounds of lard. As the surface of this ointment always grows white after a time, for which some account by ascribing it gratuitously to the absorption of oxigen by the air, he poured this ointment while still fluid into squares of paper. Some of these he placed under a jar filled with air over mercury. In twenty-four hours no absorption had taken place, yet the surface was strikingly whitened. Others he placed under the receiver of an airpump, in which he speedily made a vacuum; and this he kept up for some hours, giving occasionally a stroke with the piston, which at first occasioned an ebullition of air-bubbles. The ointment when removed from the vacuum was perfectly yellow, and remained in this state without the least change.

Whiteness of the surface owing to air detained there. From these experiments he conceives, that the white crust is owing to the extrication of gas, either nitrogen or nitrous, which arrives from all the internal parts at the surface, and increases its volume. As it gradually cools, it does not leave the gas time to escape entirely, so that part of it remains, and forms an infinite number of small white bubbles at the surface.

May be made so as to remain yellow. In confirmation of this may be added, that, when the ointment is suffered to cool in the vessel in which it was kept in fusion, and particularly when it is still heated a little, the quantity of caloric is sufficient to expel all the gas, and the ointment remains constantly yellow, without undergoing any farther alteration.

Boiled in water. To examine this compound, and form a judgment of the chemical changes, that might have taken place, Mr. Vogel boiled in water for half an hour some ointment, that had been made about two years. It became very clotty, and the water

water was so interposed, that it was difficult to separate the whole of it. The water acquired a yellow colour, and a slightly bitter taste; was scarcely at all acid; and did not contain an atom of mercury.

By way of comparison some ointment only a day old was treated with hot water as long as it would take up any thing. This had nearly the same characters as the water with which the old ointment was washed, and scarcely exhibited any traces of mercury on the addition of a hidrosulphuret.

The acid converts the mercury to yellow nitrate.

Hence it was natural to conclude, that the acid nitrate of mercury had undergone a change; and it might be presumed, that it had passed to the state of yellow nitrate, or nitrous turbith, which is little soluble in water. On keeping the ointment a long time in fusion, however, no turbith separated from it, so that it must be intimately united or dissolved in the lard. To satisfy himself of the possibility of this solution, Mr. Vogel heated turbith with oxigenized lard; and having decanted the clear fluid part, it perfectly resembled the unguentum citrinum, and contained a large quantity of mercury.

Oxigenized lard and yellow nitrate will form unguentum citrinum.

Some consider the mercury as useless.

With respect to the virtues of this ointment, which some physicians assert are the same with those of lard simply oxigenized by nitric acid, I do not pretend to decide: but it is probable, that a substance containing mercury in actual combination must produce different effects from one that does not.

Neutral nitrate changed from white to yellow by lard;

Instead of the acid nitrate employed above, Mr. Vogel next took neutral nitrate at a minimum, reduced it to a fine powder, and projected it into heated lard. Bubbles were immediately produced, and the white powder of the nitrate was soon changed to a yellow. The lard acquired a solid consistence, and contained mercury in solution.

which takes up part of its acid.

The neutral nitrate then is decomposed by lard: not that the mercury parts with oxigen to it, for it is already at a minimum; but the nitric acid quits in part the oxide of mercury, and attacks the lard, by which it is decomposed; the result of which is yellow nitrate of mercury, which in fact contains but little nitric acid.

Other metallic salts.

With the nitrates of silver and lead, and the oximuriates of platina and mercury, very little decomposition takes place,

and they do not produce the same effects on lard as the nitrates of mercury.

General conclusions.

From the facts here adduced we may infer,

1. That light, without air, turns lard yellow, and gives it an acrid, rancid taste, without acidifying it.

2. That lard yields no ammonia by distillation, and contains no nitrogen, so that it may be considered as a purely vegetable substance.

3. That in the sulphur pomatum a portion of sulphur is dissolved, without being acidified.

4. That phosphorus dissolves in it, but is quickly changed into phosphorous acid, and its acidification is increased by the contact of air.

5. That lard oxigenized by long exposure to air constantly becomes acid. That the water with which it is washed precipitates some metallic solutions, and, if distilled, gives out at last acetic acid.

6. That nitric acid forms with lard a yellow bitter substance, acetic acid and mucous acid. That the latter cannot be completely separated from it by washing; and that it is equally obtainable from suet by means of nitric acid.

7. That the oxigenized muriatic acid is decomposed by lard, which it leaves whitish, very soft, and incapable of furnishing the yellow bitter matter, when subsequently treated with nitric acid.

8. That mercury is in the metallic state, but very minutely divided, in fresh prepared mercurial ointment.

9. That in the unguentum citrinum the mercury is a nitrate with oxide at a minimum: that the white appearance of the surface is owing to bubbles of gas: and that the neutral nitrate of mercury at a minimum is decomposed in lard.

VII.

Extract of a Memoir of Mr. HAQUET, *on the Formation of Flint*[*].

Flint of recent formation.

AMONG the various proofs, which this gentleman has adduced of the recent formation of flint, we shall cite the following.

[*] Journal des Mines, No. 119, p. 405. November, 1806.

In

In the chalks of Volhynia there are a great number of flints, in the form of nodules more or less large. In one place were found two as big as a man's fist, enclosing petrified roots of wood. The author possesses one of these, the wood in which has not altered in colour, and appears to be beech. Another nodule found in the same place contains a number of splinters of wood. Petrified wood found in it.

The chalk, from which these flints were taken, analysed by Mr. Haquet, gave in 100 parts, lime 47, magnesia 8, carbonic acid 33, silex 7, alumine 2, oxide of iron 0·5. Analysis of the chalk bed.

He has likewise analysed several flints from different places, and found them containing:

Silex	92·75	92·50	92·75	97	89	
Alumine	1·10		1·50	1	2	
Lime	1·25	3	2·75	0·25	4·15	
Magnesia			0·51			
Oxide of iron	2	1·25	1	1	1·75	
Oxide of manganese		0·75				

Analyses of flints.

Mr. Haquet observes, that flints are never found at any considerable depth; and the deeper we go, the smaller and more distant from each other are the nodules. Always near the surface.

VIII.

Of the Oxidation of the Solder of Leaden Vessels used in Wash-houses; by J. C. Delametherie*.

An enlightened amateur of the arts, Mr. Fougeray de Launai, who lives near Soissons, informed me, that laundresses, who wash great quantities of linen, use for their bucking tubs large vessels made of several sheets of lead, soldered together with the common solder consisting of lead and tin. This solder suffers no damage, as long as the vessel is kept in use: but if it be left empty for a few months, the solder is so much oxided, that the lead must be soldered afresh, before it will hold lie again. I requested prof. Vauquelin to examine this solder, and the following is the result. Lead solder corroded in vessels used in washing.

He found, that it consisted almost wholly of carbonated The lead con-

* Journal de Physique, Vol. LXIII, p. 252. September, 1806.

verted into a carbonate, oxide of lead, with a few particles of iron and tin. From this, and the circumstances under which it appeared to have been formed, he concluded, that the metals forming the solder were oxided by the air through the influence of the alkali, and that at the same time the carbonate of potash had been decomposed; that is to say, the alkali had united with the tin, and the carbonic acid with the oxide of lead.

and the tin oxided and dissolved.

Oxides of tin and lead together decompose carbonate of potash. This conjecture he verified by a direct experiment, taking nearly equal parts of oxide of tin and oxide of lead, and heating them slightly with a solution of carbonate of potash. The tin was dissolved, and the lead carbonated.

VII.

Example of a Calculation in the Doctrine of Chances; a Tide Table, and Remarks on the breaking of Waves. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

A Friend of mine, who has been spending the summer at a watering place, has proposed to me some questions, which arose out of the amusements of the season, but which it required some little consideration to answer in a satisfactory manner. If you think the results of my reflections likely to be interesting to your readers, they are much at your service.

What is the chance of winning a raffle by a given number thrown? 1. The first question was how to determine the chance of winning a raffle, when you have thrown a given number. It is usual to throw three times, with three dice; the highest, or sometimes the lowest, wins; and if two or more persons throw the same number, they must raffle again among themselves. We must first calculate the chance, that none of the persons who are to throw will throw higher; and then the chance, that, if they do not, any one of them will throw the same number, or any two, three, or more; but as this calculation cannot easily be made during the time of raffling, I have made a table which is sufficiently accurate for the purpose.

A Table

Persons to throw	Number thrown.																												
	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48
1	1	2	3	5	7	10	13	19	25	32	39	46	54	61	68	75	81	86	90	93	95	97	98	99	99	..	..	..	..
2	(In 100)				1	1	2	4	6	10	15	21	29	38	47	56	65	73	80	86	90	94	96	98	..	..	..	..	..
3							..	1	2	3	6	10	16	23	32	42	52	63	72	80	86	91	94	97	98	99	..	..	..
4							..	..	..	1	2	5	8	14	28	32	42	53	65	74	82	88	92	95	97	..	..	..	..
5							..	..	..	..	1	2	5	9	16	24	34	46	58	68	78	85	90	94	..	..	..	..	..
6							..	..	..	..	..	1	2	5	11	18	27	39	52	64	74	82	89	93	96	98	99	..	..
7										..	..	1	3	7	13	22	33	47	59	70	80	87	92	95	..	..	..	..	..
8										..	..	..	2	5	10	18	29	42	55	67	77	85	91	..	..	..	..	..	..
9													1	3	7	14	24	38	50	63	75	84	90	94	97	..	..	..	..
10													..	2	6	12	21	34	47	60	72	82	89	..	..	..	..	..	..
11													..	..	4	9	18	30	44	57	70	80	88	93	..	98	..	..	..
12													..	1	3	7	15	27	40	54	68	79	87	92	96	..	99	..	..
13													..	..	2	6	13	24	38	52	66	77	86	..	..	..	..	..	..
14													..	..	..	5	11	22	35	49	63	76	85	91	..	..	..	..	..
15													..	..	1	4	10	20	32	47	61	74	84	..	95	..	..	..	..
16													..	..	..	3	8	18	30	44	59	73	83	90	..	..	..	..	..
17													..	..	..	2	7	16	28	42	58	71	82	89	..	..	..	..	..
18													..	..	..	..	6	14	26	40	56	70	81	..	94	..	..	..	..
19													..	..	..	..	5	13	24	38	54	68	80	88	..	..	..	..	..
20													..	..	..	1	4	12	22	36	53	67	70	..	..	..	..	..	..

	Number thrown.											
	37	38	39	40	41	42	43	44	45	46	47	48
21	4	10	21	34	51	66	78	87	93	97	99	..
22	3	9	19	33	49	64	77	86	..	..	..	..
23	..	8	18	31	47	63	76	..	..	96	..	..
24	2	..	16	30	46	62	75	85	91	..	..	..
25	..	7	15	28	44	61	74	..	..	..	..	..
26	..	6	14	27	43	59	73	84	..	..	..	..
27	1	5	13	25	42	58	..	..	90	..	..	..
28	..	..	12	24	40	57	72	83	..	..	..	..
29	..	4	11	23	39	56	71	..	..	95	..	99
30	..	..	10	22	38	55	70	82	89	..	..	..

To find the chance of losing, subtract the chance of winning from 100. Thus, having thrown 42, if there are five persons to throw, the chance of winning is 90, and that of losing 10; the odds being 9 to 1.

If the raffle is to be won by the lowest number, the chance may be found by subtracting the number thrown from 63, and consulting the table; thus if 21 are thrown, and there are 5 persons to throw, the chances are 90 in 100.

Universal tide table.

2. In the second place I was asked for the easiest mode of finding the time of high water, with sufficient accuracy for common purposes. I have made a table, which, I believe, is tolerably correct; but not being so conversant in the subject as I could wish, I should be much obliged to any of your correspondents skilled in navigation, if they would compare it with the best observations, and inform me whether they approve of the principle.

Waves break into surf from the upper part moving faster than the lower.

3. The third inquiry related to the cause of the breaking of a wave into surf. Waves seldom break at sea, unless the wind is very high: but when they approach the shore, they always break sooner or later. The general reason of their breaking appears to be the excess of the velocity with which the upper part of the wave advances above that of the lower part: and this may be derived either from the effect of the wind on the upper part, or from the resistance of the bottom to the motion of the lower part, or from a third cause, which is more general, where the magnitude of the wave is at all considerable in comparison with the depth of the fluid; for in this case the upper part of the wave must have a natural tendency to advance more rapidly than the lower, on account of the greater depth which determines its velocity. Beside this, the form of the wave itself, where the water is shallow, may be such as to render it incapable of advancing without a change of the direction of its anterior surface into a situation more nearly vertical.

Defect in the calculation of the velocity of waves.

In the calculations by which the velocity of waves has been determined, it has been usual to neglect not only the difference of the whole depth of the fluid at different parts of the wave's surface, but also the immediate effect of the horizontal motion of the particles, so far as it is not concerned in producing an elevation or depression by its variations. The theory, abstracted from these considerations, is perfectly correct; and may be combined with their results so as to be rendered applicable to some cases, which are not otherwise comprehended by it. Thus if we suppose a wave, terminated by two planes, equally inclined, to be placed in a surface on which it can move without resistance, it may be shown, that the highest point will begin to be flattened with the velocity deducible from the depth at that point, the new angular

angular point advancing on each side upon the inclined surface with a velocity which is at first equal to that which is due to half the depth, and is afterwards uniformly retarded; so that the angle is twice as long in travelling over the whole surface of the wave, as it would otherwise have been. The centre descends at first more rapidly than the part nearer the margin, so that the wave becomes concave in the middle, instead of being flat, as it would be if the depth of the fluid were very great. In the mean time the margins of the wave advance with a velocity, which continues to be uniformly accelerated, until the angle reaches it; and this velocity is as much smaller than that of a body falling by its weight, as the height of the wave is smaller than half the breadth: for the whole horizontal pressure acting on any vertical section of the wave is every where proportional to the quantity of the fluid beyond it, and as long as the deeper parts retain their form, they will urge forward the shallower with a constant force. But if any part of the surface of the wave be concave, the velocity thus produced in its upper parts will cause them to advance more rapidly than the lower, and the surface will become more and more inclined to the horizon: if on the contrary it be convex, the lower parts will be protruded, and the convexity will be diminished. Beside the case of a wave advancing in consequence of its gravitation on a flat shore, these considerations are also applicable to that of a drop of oil, spreading, by the force of cohesion, on the surface of a vessel of water.

These reasonings applicable to a drop of oil spreading on water.

I am, SIR,

Your very obedient servant,

10 *Sept.* 1807. HYDROPHILUS.

ANNOTATION.

MY correspondent not having gone at large into the use and application of the tide table annexed, Plate IV, a little more particular detail on the subject will probably be acceptable to many of my readers. The small shaded circle in fig. 1 being cut out, a damp wafer is to be put in its place, and over it fig. 2; which is likewise to be cut out, and

Manner of fitting up and using the tide table.

and so adjusted, that its dotted circle shall coincide with that of the same size in fig. 1. Underneath the wafer a circular piece of paper, about the same diameter as fig. 2, is to be placed, so that fig. 2, may move freely round on its centre. The oblong shaded space in fig. 2 is to be cut out previously, so that the name of the place, for which we want to find the time of highwater, may appear through it. The table being thus put together, we have only to turn round the smaller circle, fig. 2, till the name of the place, for which we would know the time of highwater, is seen through the aperture; when the time for any given day will be shown by the part of the hour-circle, fig. 2, that stands against the line of the moon's age on that day. Thus for example, if I want to adjust the table to Worthing, having turned the circle till Worthing appears through it, I shall find, that, when the moon is two days old, it will be high water there about five minutes before 12; when the moon is nine days old, about a quarter after 6; and so for any other day.

The difference of the length of the lines in the larger circle points out the progressive increase and decrease of the rise of the tide; showing its comparative height, from the spring tide, when it is the greatest, being about 36 hours after new or full moon, at every place, to the neap, when it is the least. Hence, if we know what is the general rise of the spring tides at any place, we may calculate how high the tide may be expected to flow at any given time of the moon's age.

Table to show the moon's age.

As it is obviously essential for finding the time of high water to know the moon's age, a general table of lunations is annexed. The use of it being sufficiently explained in the table itself, all that is necessary for me to say is, that the small shaded circle of fig. 4 is to be cut out, and fig. 3 to be cut out and put over it in the same manner as fig. 2 over fig. 1.

For the gratification of such of my readers as might wish to have these tables in a form for use, without taking the trouble of copying them, or destroying the plate, I have given a duplicate of it, which may be cut out and pasted on a card.

Hydrophilus will no doubt perceive, that I have taken the liberty

[redacted]

Nicholson's Philos. Journal, Vol. XVIII. Pl. IV. p. 242

Universal Tide Table.

Fig. 1.

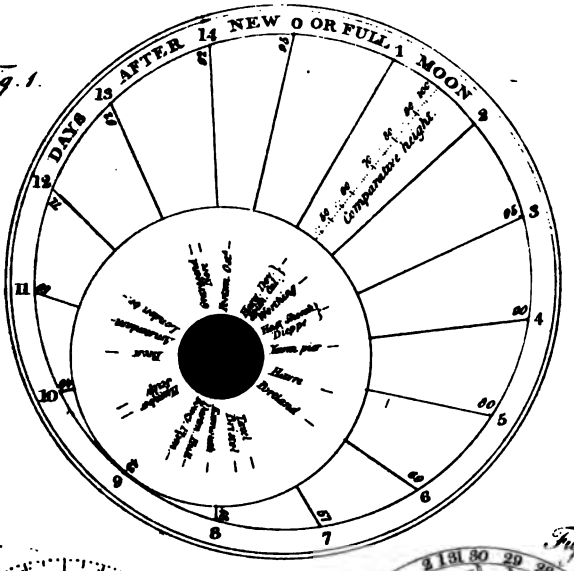


Fig. 2.

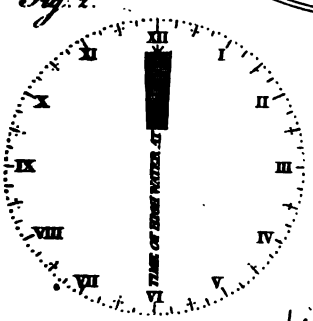
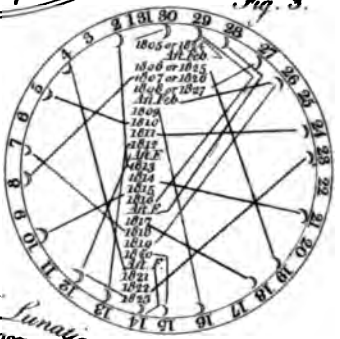


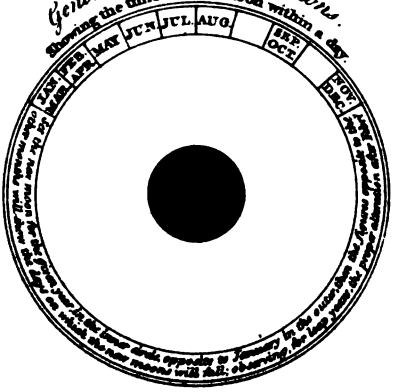
Fig. 3.

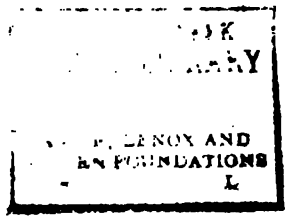


General Table of Lunations.

Showing the time of new moon within a day.

Fig. 4.




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Nicholson's Philos. Journal, Vol. XVIII. Pl. IV. p. 12.

Universal Tide Table.

Fig. 1.

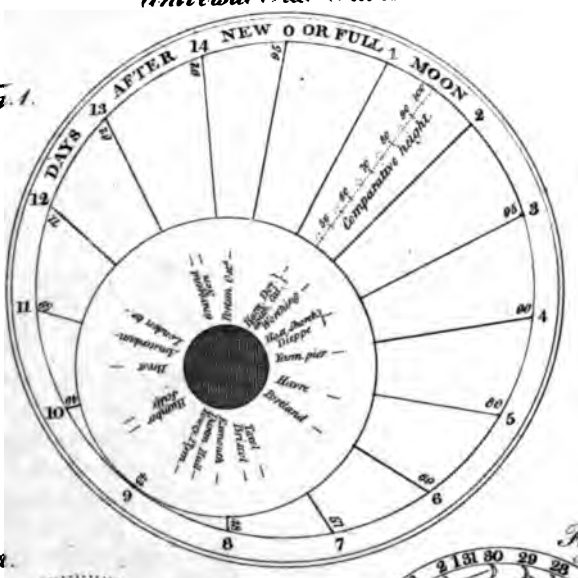
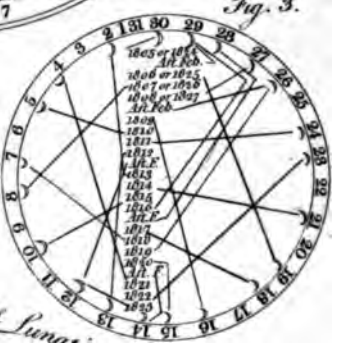


Fig. 2.

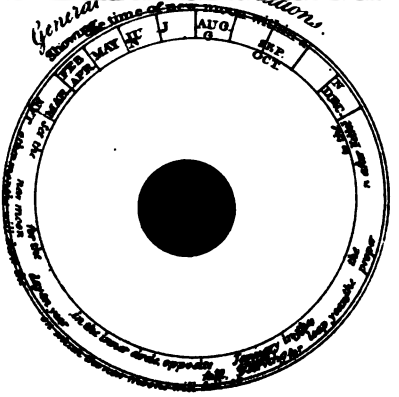


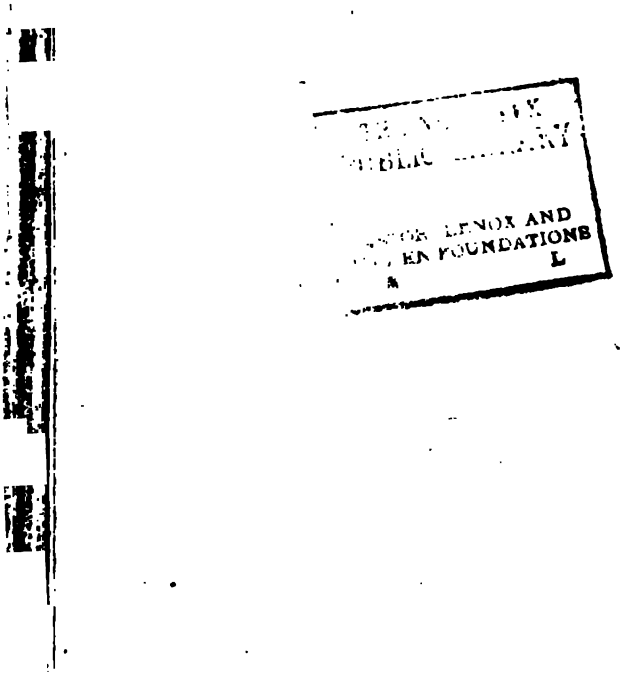
Fig. 3.



General Table of Lunations.

Fig. 4.




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liberty of making a slight alteration in the title of one of his tables. If the reason of this should not readily suggest itself to him, it will be explained in a private letter, on his favouring me with his address.

X.

Nondescript Encrinus, in Mr. DONOVAN'S *Museum.*

SIR,

PERMIT me through your excellent publication, to acknowledge my obligations to Mr. Donovan, for the advantages I have derived in my inquiries respecting the mineralized remains of the animals of the former world, from the examination of the inestimable fossils, contained in his matchless museum. Mr. Donovan's Museum.

By the investigations which I had previously made, and from specimens in my own collection, I had ascertained, that England alone yielded several species of *encrinites;* as I trust I shall show in the second volume of Organic Remains of the former World, now in the press. But by an examination of the series of fossils in this department of the London Museum as above mentioned, I have gained the knowledge, that our own country can boast of yielding at least one additional curious species of this animal, hitherto I believe unknown; and forming by the length of the arms an intermediate species between the lily and plumose encrinus. The specimen of which I speak is numbered 924 in the brief catalogue which is delivered at the museum. Several species of encrinites found in England. New species.

From another specimen in the same collection, marked 950, I also acknowledge having derived very considerable information respecting the structure of that wonderful lost animal, the *tortoise encrinus.* Tortoise encrinus.

Having no reason for concealing any of the motives which induce me to trouble you with this request, I do not hesitate to avow, that one of these is a wish to call the attention of the curious, as well as scientific, to the most complete collection of British Natural History, which has ever yet been

been formed; a museum not confined to any one particular branch, but comprehending alike the three great departments of nature, the zoological, botanical, and mineral productions of the island, upon the grandest scale possible. It will not be too much to say, that this museum, from the science evinced in its arrangement, independent of its importance as a collection of choice and valuable specimens, must, to those desirous of such knowledge, prove a most instructive school; and afford an inexhaustible fund of information to all those, who think the Natural History of their own country worth attending to.

I am, SIR,

Your most obedient servant,

JAMES PARKINSON,

Hoxton Square,

Author of "Organic Remains," "Memoranda Chemica," &c.

Mr. NICHOLSON.

Sept. 6th, 1807.

XI.

Inquiry respecting a Fact not hitherto noticed in the way of discussion. In a Letter from R. B.

To. Mr. NICHOLSON.

SIR,

Sediment of water thrown into undulation arranges itself in parallel ridges.

WHEN turbid water has become clear by subsidence, the light stratum of earthy matter, which covers the bottom of the vessel, is often, as might be expected, distributed without any particular appearance of symmetry: but if the vessel be slightly moved horizontally, so as to produce an undulation of the fluid, without much disturbing the deposit, this matter is found to arrange itself in a number of parallel ridges or embankments; or at least to indicate a manifest tendency to form such ridges. I find a great difficulty in accounting for this fact. It seems to suppose alternate differences in the velocity of the water, as it runs over the bottom. These, however, seem incompatible with the almost total want of elasticity in water.

Why?

Perhaps

Perhaps the hypothesis of a number of small eddies, or rolling cylinders of water, may account for it, as the ridges are at right angles to the course of the undulation. In this supposition, however, there appears too much of gratuitous demand.—Have the goodness to propose the matter to the consideration of your correspondents, if you should think as I do, that no fact can be too trifling for philosophical discussion.

I am, SIR,

Your obliged reader,

R. B.

XII.

Queitions on some Appearances of the Electric Spark: by a Correspondent.

SIR,

I Wish to be informed if it was ever remarked, that, when an electrical spark is taken from the conductor of a machine, the line of white light is interrupted, and the spark becomes red. Sometimes it assumes the form of two cones, one proceding from the conductor, and the other from the body which is applied to it: at other times the interruption is next the conductor; and again it will be perceived nearest the body receiving the spark. When the spark is taken at the least distance possible, the light is sometimes red, sometimes white; and when the spark is some inches in length, the interruption is perceived in two or three places of the line of white light. Any explanation of these phenomena will much oblige,

Question respecting various appearances of the electric spark.

SIR,

Your humble servant,

TYRO.

XIII.

Extract of a Letter from Mr. BIOT *to Mr. Berthollet*.*

Tarragona, 20th December, 1806.

I Have had an opportunity of conversing with that excellent observer, Mr. de Marty, on several subjects of experi-

* Annales de Chimie, Vol. LXI, p. 271, March, 1807.

ments

ments in which he has long been engaged; and I have requested his permission to communicate to you the results, persuaded that you will consider them extremely interesting.

Influence of time in diminishing the elasticity of fluids.

The experiments of which I speak have for their object in the first place the influence of time on the exercise of chemical actions, when these actions tend to deprive an elastic fluid of its elasticity.

Oxigen gas absorbed by water.

1. Into a flint glass phial, the stopple of which was ground with emery, and fitted perfectly tight, Mr. de Marty introduced a certain quantity of oxigen gas, and a certain quantity of rain water, boiled or unboiled. Supposing there is but a small quantity of water, on shaking the bottle for a few minutes a certain portion of the gas will be absorbed, as may be found by opening it under water. After it has been thus shaken and opened several times, the water in the phial will be saturated, and absorb no more.

Left standing on it in a close vessel more will be absorbed.

When things are in this state stop the bottle close, and put it away in a place shaded from the sun, observing at the same time the state of the barometer and thermometer. After it has stood thus two or three days, shake the bottle again, open it under water, and you will find the water rise into it a little. Stop the bottle, put it into its place, and shake it in the same manner from time to time. You will always find a fresh quantity of gas absorbed, and the effect will be the more perceptible, the longer you leave the phial before you make the experiment.

In 18 months half the bulk of the water absorbed.

I myself was witness to these effects. Mr. de Marty had the civility, to open under water before me a phial, that had been kept stopped for more than a year and half, and which contained oxigen gas with a small quantity of water. The water rose in it very perceptibly, and the absorption appeared to me equal at least to half the quantity of the water, that the bottle contained before it was opened. The barometer and thermometer were both nearly at the same height as when the bottle had been set by, and the water in the trough was at the same temperature.

At first the gas combines feebly;

From this experiment it appears, that the same volume of water, which at first was able to absorb only a certain portion of oxigen, absorbed by the assistance of time a more considerable

considerable quantity. Hence it would seem, that in the first case the air was but feebly combined, and in some sort interposed between the particles of water: but the continued action of the liquid, diminishing the elasticity of the gas more and more, and contracting its dimensions as it were by degrees, occasioned it to enter farther within the sphere of attraction of its particles, which rendered the water capable of absorbing a fresh quantity of gas.

but its elasticity diminishing, the union is more intimate.

2. The same thing takes place with regard to hidrogen gas, and Mr. de Marty afforded me the pleasure of witnessing this likewise. The absorption was equally great. He finds by his experiments, that this gas is absorbed in larger quantity and with greater promptitude than oxigen gas. He finds also, that the bulk of the gas absorbed is not equal to that of the water in two years.

Hidrogen gas affected in the same way, but water absorbs more, and in less time.

3. Water already loaded with oxigen is better adapted to absorb hidrogen, and the contrary. This is analogous to what von Humboldt and Gay-Lussac have observed, but the experiment of Mr. de Marty has the advantage, like the preceding, of having been made in close vessels.

Water loaded with the one absorbs the other more readily.

4. The absorption is so much the more sensible as the water is more considerable, and is proportional to it.

Proportional to the quantity of water.

5. These effects do not take place with nitrogen gas. After the water has been once shaken for some time with this gas, it will not absorb an atom more, however long it be left in contact with it.

Only a limited portion of azote absorbed.

6. If water loaded with nitrogen be placed in contact with hidrogen or oxigen gas, it will absorb it, without parting with its hidrogen. If it have been supposed, that an exchange takes place, it is because in fact a little nitrogen escapes at the commencement of the absorption of the hidrogen or oxigen: but on shaking the water and the gasses together, all the nitrogen, that was before interposed between the particles of the water, will enter into it again as before, independent of the hidrogen or oxigen absorbed.

Water saturated with azote still takes up hidrogen or oxigen; without ceding its place.

7. The preceding result is so true, that an accurate analysis of atmospheric air may thus be made by the absorbent action of water alone. To effect this it is sufficient, that the water be previously impregnated with nitrogen; when it will absorb exactly 21 hundredths of the volume of the atmospheric

Thus atmospheric air may be analysed by water.

mospheric air in contact with it, precisely as a sulphuret would do. Mr. de Marty asserts, that water thus employed in large quantity, to prevent the process from being too tedious, is an excellent eudiometer, and he has had recourse to it repeatedly. If you have no nitrogen at hand, water may be impregnated with this gas by shaking it in contact with atmospheric air, and leaving it some time in contact with it. By these means it absorbs all the nitrogen it can contain, and the oxigen it takes up with it does not prevent it from absorbing in time, according to the first experiment, that of the air to be analysed. Mr. de Marty avails himself of this absorbing property of water to ascertain whether oxigen gas contain any nitrogen; for, if it do, water saturated with nitrogen will not absorb the whole.

8 It is long since Mr. de Marty was acquainted with many of these facts. Some of them, particularly 6 and 7, were known to him, when he composed his Memoir on Eudiometry; but he contented himself with simply mentioning the property he had observed in nitrogen.

Does the oxigen form an acid?

Does this oxigen, continuing to be absorbed, form at length an acid? and if so, what acid is it? The solution of this problem Mr. de Marty awaits from time and experience.

The experiments were made with great care.

With respect to the preceding experiments I shall add, that they were all made with the greatest care, in vessels well closed; that Mr. de Marty has repeated and varied them in a thousand manners; and that he appears to have observed the most scrupulous accuracy in all.

Mr. de Marty's memoir on eudiometry.

I shall conclude this letter with some remarks respecting the Memoir on Eudiometry formerly published by Mr. de Marty, of which I have a copy before me in the Spanish language, in the *Memoriale Literario* for 1795; and of which there is an abstract in the Journal de Physique, year 9. In this abstract, however, many experiments have been omitted; the connexion and detail of which were indispensably necessary to understand the course of the author, and the conclusions at which he arrived: so that in consequence of this omission opinions have been ascribed to Mr. de Marty contrary to those he held, and results the reverse of those he sought to establish.

For

For instance, in your Chemical Statics you seem to insinuate, that Mr. de Marty ascribes to sulphurets the property of absorbing nitrogen from their nature; and von Humboldt and Gay-Lussac in their work on Eudiometry express this opinion still more affirmatively. This errour arose from the abstract. Mr. de Marty says expressly in his paper, that a hot sulphuret acts as any other liquid would do, that had been deprived of the quantity of nitrogen it is naturally capable of absorbing; and thus he accounted for the variations he experienced on operating with greater or smaller quantities of sulphuret. Mistaken by Berthollet, and others. Sulphurets have no affinity for nitrogen.

Von Humboldt and Gay-Lussac say too, that Mr. de Marty fixes the proportion of oxigen in the atmosphere to be between ·21 and ·23; and hence they draw an argument against the method of operating with sulphurets, which in consequence appears to them much less accurate than the proof by hidrogen gas. But this uncertainty between ·21 and ·23 Mr. de Marty experienced only in his first experiments: and it was from this very variation, and a wish to diminish its extent, that he was led to discover the errour occasioned by the absorption of nitrogen by the sulphuret, when it is deprived of this gas by heat; so that when he had completed the improvement of his method, the results were constantly restricted between ·21 and ·22. Proportion of oxigen in the atmosphere.

This being once thoroughly proved, I do not see what advantage the analysis by hidrogen gas has over that by the sulphuret, when it is performed with due precaution, which should always be a matter of course. It certainly has not the advantage of requiring less time; for by operating as Mr. de Marty does, any one who has acquired a little habit of making the experiment can perform this in five minutes. It has not that of greater simplicity: for the sulphuret requires only a graduated tube, and a ground stopple bottle; while for the hidrogen gas at least a small eudiometer and an electrophorus are necessary; and what is very inconvenient, the latter must be kept in a state to give sparks, which is not very easy on mountains and when travelling, particularly if the air be loaded with moisture. Lastly I will add, that it has not the advantage of superior accuracy: for when it is once proved, that the sulphuret will absorb only As a test of it, hidrogen not superior to a liquid sulphuret in requiring less time, being more simple, or giving a more accurate result.

only a given quantity of nitrogen as a liquid, and that, if it be taken thus saturated; it constantly gives the precise proportion of ·21 in close vessels, no objection can be brought against its use; while a very strong one may be urged against the use of the electric spark, since either boiled water is employed, and then it will absorb a small portion of the oxigen very greedily, or the water will be saturated with air, and then the pressure occasioned by the detonation will always force out some bubbles. One or other of these inconveniences appears to me unavoidable; and the latter in particular often teased me, in the numerous experiments I had occasion to make on the analysis of gasses by the electric spark, either with Thenard, or alone, on the Alps. It is true the differences hence arising, when we operate with care, amount only to some thousandth parts: but it is of thousandth parts we are speaking; and if there be another process, which gives at least equal if not superior accuracy, with less trouble, it appears to me to deserve the preference.

But objectionable, as boiled water absorbs some of the oxigen, and unboiled gives out air.

Finally, to return to the memoir of Mr. de Marty, I shall add, that he has equally tried the air contained in theatres and in churches, when a great concourse of persons were assembled in them, and that he constantly found the same quantity of oxigen; an experiment which von Humboldt and Gay-Lussac made likewise at Paris.

Air in crowded churches and theatres not deficient in oxigen.

XIV.

Summary Considerations on the Prismatic Colours of Bodies reduced to thin Pellicles; with an Explanation of the Colours of Annealed Steel, and those of the Peacock's Feathers. A Fragment of a Work on Colours: by C. A. PRIEUR*.

CERTAIN extremely thin substances, the thickness of which varies progressively from one part of them to another, exhibit, as is well known, a series of colours of different

Thin substances exhibit prismatic colours.

* Annales de Chimie, Vol. LXI, p. 154, Feb. 1807.

tints,

tints, sometimes very brilliant. It is not my intention here to describe these, still less to dispute the particulars so admirably described by Newton. I shall only attempt to draw some conclusions respecting the origin of these colours, to establish a comparison with those arising from absorption, and to assign the true cause of some phenomena hitherto differently explained.

The principal effects, to which it is of importance for these purposes to call the attention, are the following.

When the light falls on very thin bodies, that exhibit the prismatic colours:

When light falls on these

1. At the places where these colours arise on the thin substance, each pencil of rays, or if you please the white light, is separated into two portions in a variable manner, and one of these portions is reflected, while the other can issue from the substance only by transmission.

it is partly reflected, partly transmitted.

2. This division of the pencil varies according to a certain law, which depends on the thickness of the body, its density, and the inclination of the luminous rays.

This depends on its thinness, density, and inclination to the ray.

3. Each ray in particular comports itself, as if it possessed the singular property of having fits of easy reflection at periodical intervals, and fits of easy transmission at other intervals alternating with the former. These various results are equally indisputable.

Each ray has alternate fits of easy reflection and transmission.

But whence can this disposition of the rays arise? Newton has considered it as inherent in the rays themselves, not only in that part of their passage comprised between the two extreme surfaces of a body that they traverse, but throughout the whole course of these rays, from the moment they begin to issue from a luminous body*. This is a kind of occult cause, of which it is difficult to form a clear idea; and accordingly some distinguished philosophers have shown great hesitation to admit it.

Newton supposes the cause of this inherent in the ray itself, in one place:

But Newton himself, at the end of his work, puts us into the right road in a more happy manner, when he asks, whether it be not by virtue of the same principle, that the rays are reflected and refracted by bodies, and inflected in their vicinity †.

in another refers it to the same cause as its reflection.

* Opt. lib 2, part 3, prop. 13. † Ib. lib. 3, quæst. 4.

It is much to be regretted, that this great man did not treat the subject of inflexion as extensively as he did that of coloured rings; or even that he did not attend to the deviation of light in the vicinity of bodies, before he examined its changes of direction by the action of their surfaces: as unquestionably he would have deduced new and very valuable consequences from it.

A great analogy between these phenomena.

In fact the greatest analogy subsists between the phenomena of inflexion round a minute body, and those of reflexion or transmission by thin substances: for the coloured fringes in one case appear to follow the same law as the coloured rings in the other. And if this be not very sensible with respect to the fringes adjacent to the shadow of a body of small diameter received into a dark chamber, it is more evident in the fringes produced by the light that passes between two bodies very near together; it is still more in the series of coloured images formed between the plumes of a feather, when looking at a candle through them; and it is very manifest likewise in the bars seen by the eye, when a piece of linen, or a series of wires very near together, is placed between it and the light, as in the experiments of Mr. Rittenhouse.

Coloured fringes and coloured rings follow the same law.

Method of rendering this more obvious.

I have found a method of rendering this resemblance still more conspicuous. For this purpose I employ black crape. If the eye be covered thus, and from a dark place you look at a light a little distant, you will perceive the light surrounded by a series of very apparent rings, the colours of which are very vivid, and of the same tints as those of the coloured rings of thin plates.

Candle seen through vapour.

If the flame of a candle be placed in the midst of a pretty abundant aqueous smoke, or so that it can be seen only through this smoke, the flame will appear surrounded by perfectly analogous rings. I can imitate them likewise very conveniently by tarnishing a glass with breathing on it, and immediately looking at the image of a luminous body either through it or reflected from it. Those rings, which are sometimes seen surrounding the sun or moon very closely, are probably phenomena of the same kind.

Newton speaks of serpentine undulations.

On the other hand Newton speaks of undulations like those of an eel, which he suspects are produced in the rays, when

when they pass very near bodies*. The formation of these, and the necessity of their existence, I think I can render sensible.

How these are produced.

With this view I would call the attention to the very interesting results of the experiments of Newton and s'Gravesande relative to inflexion: results so certain, that no one certainly will attempt to question them, but which it is notwithstanding satisfactory to be able to verify ourselves, and to observe with all their peculiarities, as I had the advantage of doing in experiments on the same subject made at Mr. Tremery's, in concert with Messrs. Berthollet, and the particulars of which I have given in a preceding part of my work.

Each particle of matter surrounded by an attractive sphere interiorly, and a repulsive sphere exteriorly.

From the action which a point, or the edge of any body, exerts on the luminous rays, it seems to me we are authorized to consider each molecule, or distinct parcel of matter, as enveloped with a double sphere of activity in respect to light: one more interior, in which the rays are attracted by the body; the other more exterior, in which the rays are repelled. Now it will happen, that, in several positions, a ray, coming to traverse the repellent sphere, will describe there a curve convex toward the body; that, if it afterward penetrate the attractive sphere, the curve of deviation will be concave toward the body; and that it will a second time become convex toward it, when the ray repasses into the sphere of repulsion, to continue its course. Here we have the commencement of an undulatory motion, the curves of which may be multiplied by a series of molecules.

Is this the cause of the fits of easy reflection & transmission?

Would this cause be sufficient, to effect the fits of easy reflection and transmission of the rays directed to the surface of a body?

Probably it is.

The phenomena of colouration here considered appear to me, to be very naturally explained by this simple mean: yet I merely announce it as a probability. To leave nothing to be desired in such a proposition, no doubt more profound investigation is necessary; as well as in particular to put it to the test of calculation, in order to see, whether it be possible by the attractive and repulsive powers ascribed

* Opt. lib. 3, quæst. 3.

to each molecule of a body, in a case given to deduce the motion of the luminous rays repelled or impelled now in one direction, now in another, conformably to the reflections or transmissions produced by pellicles.

The same colours produced between bodies in a vacuum.

It is of more importance to my object however to remark, that the colours arising from fits of easy reflection and transmission are equally produced, as Mazéas very justly observed, between the surfaces of two bodies brought near together, without the interposition of any matter, as in two lenses, or two pieces of glass, applied to each other in the vacuum of an airpump.

And in thick plates.

On the other hand these colours do not always require a very small distance of the surfaces, since Newton himself obtained coloured rings by the action of two surfaces of a concave glass mirror three lines thick; and found, that in thick plates these rings depend on the ratio of the thickness, according to the same law as he had determined with respect to thin plates; which he confirmed by the observation of the rings of a mirror only one line thick.

Colours of pellicles independant of the substance itself;

and of its thinness.

We see then by comparing the various phenomena I have mentioned, that the prismatic colours of a pellicle, or a thin plate of glass, are as fugitive and independant of the proper colour of the substance, as those of a thick piece of glass: that those colours even may not depend on the thickness in any respect, as when they arise in the interval between two glasses brought together, or in the fissures of certain minerals: that they have the greatest analogy with the rings produced in a mist, in smoke, or in the intervals of threads impermeable themselves to light: and finally, that, if we trace it up to the action of a point, or a single particle of matter, on the luminous fluid, we shall there find a very probable origin of the modifications of the direction of the rays, that are deflected by the particles of bodies in the different instances quoted, and which, being differently influenced each according to its nature, ultimately escape in a different direction. Hence results a variety of colours on these bodies, determined solely by the number or distance of their particles, without any relation to their nature.

Analogous to the rings in vapour, and between opake substances.

And may be traced to the action of particles of matter on light,

without any relation to their nature.

Colours of bo-

Let us now proceed to establish a parallel between these

sorts

sorts of colours, and those of the particles of bodies subjected to the laws of absorption. dies that absorb light.

In the first place with respect to the latter the luminous pencil is not divided as in the former. The rays that do not reappear in a given direction are not thrown into another direction; they remain absorbed in the substance, even when the mass is perfectly transparent. The pencil not divided here.

In the next place the colours resulting from absorption are sometimes owing to groups of rays very different from those that thin pellicles can furnish. For instance, these never produce a compound colour like that of bodies tinged violet by oxide of manganese, or like the blue of cobalt or of indigo. Besides in these two kinds of phenomena there is no relation between the progress of colour depending on the degree of thickness. The colours differ from those of pellicles.

Thirdly the colours of the thinnest pellicles are very vivid. Those of the most intensely coloured solutions on the contrary are imperceptible when so thin. It is for this reason the colour of extremely thin leaves of mica has no relation to the yellow or other colour of the mass from which they are separated; they resembling pieces of the most colourless glass of similar thinness, so that mixed together they would not be distinguishable. Are imperceptible when the body is thin.

Thus glass, mica, or any other substance, which when very thin is invested with the most brilliant colours, passes to a colourless state by increasing its thickness, or to a colour independant of that displayed by it when thin. Substances coloured when thin, colourless when thicker.

But it may be said, to compare a coloured mass to an assemblage of parcels of a determinate thickness, these parcels must be kept at a suitable distance from each other. Molecules of a given thickness, kept at a given distance,

In this case, I should answer, you will have a certain colour reflected, and another transmitted, which is precisely complementary to the former. Now this double colouration never takes place in perfectly diaphanous substances. would reflect one colour, and transmit its opposite.

The examples of the infusion of nephritic wood and precipitates of gold are not more applicable to this case, since, as I have shown, the reflected colours are owing to particles impermeable to light, and disseminated in a transparent fluid; and we may alter the nature of these particles, or even have Infusion of nephritic wood & precipitates of gold, opake particles in a transparent fluid.

have others, so as to change the reflected colour, without any alteration in the transmitted colour.

Hitherto there is no case known, that allows us any foundation to consider a body that is perfectly transparent, or even a little turbid, as composed of parcels of a given thickness, and kept at a necessary distance, in order to produce a colour dependant on the thickness of its elementary parts.

Colours of pellicles varied by the inclination of the rays, and by the adjacent medium.

Lastly the colours of pellicles are in certain cases variable by the inclination of the light and of the eye, and sometimes too by the influence of the mediums with which they are in contact. Nothing similar to this takes place in the colours proper to the particles of bodies; for these are fixed and permanent in whatever direction we look at them, and are equally unchangeable by immersion in a different fluid medium of less or greater density.

Permanent colours therefore have a different cause.

These characteristic differences I conceive are sufficient to authorize the opinion, that the colours of substances in masses have not the same origin as those of thin pellicles; a conclusion as important with respect to its object, as to the difference of opinion that still subsists on it among the learned *.

I shall conclude with some observations on two curious kinds of phenomena, analogous to the subject, which I think I have sufficient grounds to explain in a manner different from that generally admitted.

Colours of heated steel, referred to the same cause by Newton;

The first relates chiefly to the colours of annealed steel. Newton has ranked these among those that depend on coloured rings; not from a particular examination, but simply as a consequence of the system he had formed, supposing that the magnitude of the metallic particles must have been altered by the action of the fire. He did not consider whether there were any other causes, between which a choice was to be made.

to oxigenation by the moderns.

More modern philosophers on the contrary have ascribed these colours without any hesitation to a different degree of

* See among others in the 2d edition of Berthollet's Elements of Dyeing, and the 2d edition of Haüy's Treatise on Natural Philosophy, the discussions and opposite opinions of these celebrated authors on this question.

oxidation,

oxidation, because they have supposed they observed a great similarity between the appearances in question, and those of several metals placed in circumstances under which they are actually oxided. This subject however deserves at least a closer examination, and the following is the simple method I have pursued.

I held a steel watchspring across the flame of a candle a few seconds in a fixed position. After it was cold and cleaned, I found both on the right and left of the central point, where the flame had been, a series of colours more and more faint [*degradées*], with periodical recurrences, such as would have been exhibited by a small band cut precisely from the middle of a circle formed of a series of concentric coloured rings. The nature of the phenomenon then is very distinctly shown here, particularly as the exterior ring was nearly 3 cent. [11·7 lines] in diameter, and the others decreased interiorly with intervals of a few millimetres. Nothing was wanting to have completed the circles, but to have operated on a broad plate of steel suspended horizontally over the point of the flame.

A steel watchspring heated by the flame of a candle acquired the prismatic colours apparently in concentric rings.

Not having such a plate at hand, I took a sheet of tin, which, with appearances analogous to the preceding, afforded very vivid colours in consequence of its natural whiteness. With a proper degree of inclination the colours are most lively; particularly the yellow, red, and blue, which form together a spot, in which the blue occupies the centre, surrounded by the red, and beyond this with the yellow, with the intermediate tints and gradations.

A sheet of tin shows this more plainly.

There is nothing in the property tin has of being oxided and forming salts, that indicates colours corresponding with these: on the other hand the periodical recurrences on the steel spring evidently belongs to a series of rings: we must conclude therefore, that this phenomenon is simply of the class of coloured rings.

Not from oxidation.

Another trial made with a gold ring equally produced repeated traces of rings, and here the suspicion of oxidation will hold still less.

Gold the same.

A copper wire gave me similar indications, though more faintly; but I have observed them very striking on copper chimneys of stoves. Lead that has just been melted exhibits

Copper and lead similar.

bits the same kind of colours, according to the circumstances of its cooling, even on its lower surface not exposed to the air.

Caloric has not altered the dimensions of the particles,

With regard to the manner in which caloric acts on the metal in these instances, I will not say, that it has altered the magnitude of the particles; for how can we conceive, that a substance can vary the disposition of the constituent elements of its molecules without changing its nature? But

but separated them progressively.

I can more readily conceive, that there has been a progressive separation of the particles, increasing from the part scarcely heated to that in immediate contact with the flame. This separation, from the principles I have laid down, must in fact have been sufficient to produce these rings.

Similar effects from altering the arrangement of the particles of bodies.

Besides, we daily see many examples of this sort of colours, where the arrangement of the particles appears to be the sole determining cause. Such are the spots formed on knife blades by the acid on fruit; those on silver by sulphurous vapours, or the continued contact of certain substances; and the prismatic colours of pellicles formed on the surface of liquids containing some matter at first dissolved, and afterward precipitated slowly by the gradual evaporation of some volatile principle, seen in manufactories and laboratories. The waters of dunghills are sometimes covered with similar colours.

Proof of this.

Now all these effects, whatever be the nature of the primitive matter, or of that secondarily formed, depends only on the arrangement of the parts. Nothing is more easy than to ascertain this. Scratch the surfaces, break the pellicles, and all this multitude of colours will be annihilated, the fragments of each leaving only particles uniformly possessing the common properties of their kind.

These effects shown in a striking manner.

The following among others is a very convenient mode of observing these effects. Take a small quantity of Scheele's green, dissolve it in an acid, and, after having largely diluted the solution with water, precipitate by an alkali, and add ammonia merely to redissolve the precipitate. Let the whole stand in a vessel not closed, and in a few days the surface will be covered with a very evident coloured pellicle, in which periodical recurrences of rings will be distinguished, if it have remained undisturbed. This pellicle may be taken

up

up by sliding underneath it a piece of paper, or of glass, as the side of a funnel for instance, to allow the water to run off. The colours of the pellicle will continue visible after it is removed; and by letting the matter dry slowly we may preserve them for an indefinite time with all their vividness. But if we pass the finger lightly over it, we shall collect nothing but a green powder: the red, yellow, blue, and purple, that appeared so brilliant, will be gone in an instant.

Changeable colours of feathers.

My second phenomenon respects the changeable colour of several parts of the plumage of the peacock, and of some other birds, as the cock, pigeon, duck, and turkey. Here, after a very deliberate comparative examination, and the most circumspect reflection, I have ultimately relinquished the idea, that these colours are to be referred to rings. My conviction was produced as follows.

Not owing to tenuity.

In the first place I considered, that these colours were not the necessary result of a certain tenuity of parts; for on the one hand several animals incontestably display in the slender filaments of their hair, feathers, or down, various degrees of tenuity, from the most imperceptible, without its producing colour. Is not the white peacock itself a striking instance of this?

On the other hand have not many birds and insects colours unchangeable in situation and in their reflections in every point of view? Those of the wings of some butterflies are perfectly fixed, though dependant on a down so fine as to be scarcely visible. It is likewise proper to remark, that all these colours indicate opacity, as those of the peacock's feathers; the wings of flies excepted, in which tints analogous to coloured rings are observed: but these membranes have a sensible transparency, like scales of mica or blown glass.

All opake except the wings of flies.

Feathers of the peacock's tail.

I afterward observed the change of colour of several feathers of the birds mentioned. In those of the peacock's tail, we see the lateral filaments, on changing their position, pass suddenly from red to green. The red is produced by a nearly perpendicular reflection of light, the green by a very oblique reflection; and there is no alternation of reflection

Red by perpendicular, green by oblique reflection.

flection and transmission, the opacity of which I have spoken not admitting it.

Eye of the feather. Near the eye of the feather an external ring exhibits yellowish tints by perpendicular reflection, and greenish by oblique reflection; while more interiorly, with the same change of obliqueness, a space of the most vivid green takes the new tint of violet. These are the principal mutations of the colours, consisting only in two tints for each place.

Pigeon's neck the reverse of the peacock's tail. In a feather from the neck of a pigeon, the disposition is the reverse of that of the lateral filaments of the peacock's tail feathers: that is to say, under similar circumstances, one of these feathers appears red, the other green, and *vice versa*.

The variation confined to two colours. This alternation of colours, confined to two principal kinds, is very difficult to reconcile with that variety of tints, which coloured rings apparently should exhibit in a substance of so little density as feathers. And if it were attempted to be supported by the more sensible changeability of tints in the pigeon's feather, this would arise from a deception; for this changeability is owing to the naturally curved state of the feather, since it ceases when the feather is straightened against a flat surface.

Feather of a duck's wing. But the feather of a duck's wing exhibits an appearance totally dissimilar. Here the transition is from green to blackish; and this green is not perceptible except in particular positions, in which the incidence and reflection of light take place under very unequal angles, as for instance, when the feather is seen with a certain degree of obliquity, the spectator's back being turned to the light. Do we meet with any thing like this in the succession of coloured rings?

Eye of the peacock's feather wetted, showed new colours, Lastly I bethought myself of wetting with caution different parts of the eye of my peacock's feather. I then saw, not a weakening of the former tints, but new colours brought out with great vividness. Desirous of knowing whether I could not produce permanent alterations by some solvent, I wetted it successively with saliva, vinegar, muriatic acid both dilute and concentrated, ammonia, ether, alcohol, and deliquesced muriate of lime: and I found, that they produced no effect except as fluids, and all nearly

which were the same with various fluids,

equal,

equal, except the concentrated muriatic acid, which showed some difference; and all their effects ceased nearly alike on drying. and disappeared on drying.

When the exterior orbit of the eye was wetted, its yellowish colour became of a lively blood red; and the reflection, which was before green when seen obliquely, was almost destroyed. If the green space within were wetted, it was the violet reflection that disappeared. Lastly with smoking muriatic acid this green space viewed perpendicularly exhibited a yellow inclining strongly to red, and the oblique reflection changed at first to green, and then it passed on to violet: none of these changes however continued permanent. The colours thus produced.

By wetting in this manner the extremity of the feathers of a turkey's tail, I brought out very vivid new colours, which could not be perceived before in the same direction; but the existence of which was indicated by certain reflections when viewed with the back to the light, analogous to those I mentioned in speaking of the duck's feathers. Feathers of a turkey's tail.

It was impossible for me, after all these particularities, to persist in classing together the changing colours of feathers, and those of the coloured rings of pellicles. An often repeated examination of them at length suggested the idea, that they might arise from the superposition of different coloured substances, sometimes of two only, or of three, or of a greater number; nearly as if, wishing to paint a body with several colours, we were to cover it in succession with a stratum of each of the proposed ingredients. Perhaps from the superposition of different coloured substances.

This supposition, suitably adapted to each part of the feathers, very well accounts for all the appearances observed. In fact, if over a coat of green paint for instance we spread a thin stratum of a violet powder, it is obvious, that on looking directly at it it will appear almost wholly green; while on lowering the eye, so as to view it obliquely, the violet will gradually predominate, till at length it alone is seen. The intermediate tints will be different shades of green, to which various shades of blue will succeed, before the violet will appear. This illustrated.

If in addition to this the green itself be laid on a red ground, this may remain invisible in the intervals of the coloured matter of the superior strata: but if these strata be rendered

rendered transparent by imbibing a fluid, the influence of the lower stratums will be perceived, and will naturally show itself here by a yellow or reddish colour, when seen perpendicularly, while its oblique reflections will exhibit greenish or violet tints; and the drying of the substance will restore every thing to its former state.

Such in reality are the variations of the tints of certain parts of a peacock's feathers: and such in my opinion is the probable cause of their formation, which is equally applicable to those of the cock, the pigeon, and several other birds, as well as to insects, and in particular that splendid butterfly, the large wings of which exhibit a fine green, when viewed perpendicularly, and when obliquely a beautiful violet.

XV.

Account of a Fulminating Compound of Silver, of a white Colour and Crystalline Appearance: by Mr. DESCOTILS*.

Detonating powder lately sold at Paris.

A Detonating powder has been sold lately at Paris as an object of amusement. It is enclosed between the folds of a card, cut in two lengthwise; the powder being placed at one end, and the other being notched, that it may be distinguished. If it be taken by the notched end, and the other be held over the flame of a candle, it soon detonates, with a sharp sound, and violet flames. The card is torn, and changed brown; and the part in contact with the composition is covered with a slight metallic coating, of a grayish white colour.

A compound of oxide of silver, ammonia, and vegetable matter.

Having been consulted on the nature of this substance, which is sent to Paris ready prepared, I convinced myself by various trials, which it is unnecessary to relate, that it is a compound of oxide of silver, ammonia, and some vegetable matter; so that it is analogous to the fulminating mercury of Mr. Howard.

Methods of making it.

This compound, which I call detonating silver, to distinguish it from the fulminating silver of Mr. Berthollet, may

* Annales de Chimie, vol. LXII, p. 198, May, 1807.

be

be made by dissolving silver in pure nitric acid, and pouring into the solution, while it is going on, a sufficient quantity of rectified alcohol: or by adding alcohol to a nitric solution of silver with considerable excess of acid.

Pour nitric acid on silver, heat it gently, & add alcohol.

In the first case the nitric acid, into which the silver is put, must be heated gently, till the solution commences, that is till the first bubbles begin to appear. It is then to be removed from the fire, and a sufficient quantity of alcohol is to be added immediately, to prevent the evolution of any nitrous vapours. The mixture of the two liquors occasions an extrication of heat; the effervescence quickly recommences, without any nitrous gas being disengaged; and it gradually increases, emitting at the same time a strong smell of nitric ether. In a short time the liquor becomes turbid, and a very heavy, white, crystalline powder falls down; which must be separated, when it ceases to increase, and washed several times with small quantities of water.

Decant, and wash the precipitate.

Or to a heated solution of silver add alcohol.

If a very acid solution of silver previously made be employed, it must be heated gently, and the alcohol then added. The heat excited by the mixture, which is to be made gradually, soon occasions a considerable ebullition, and the powder immediately precipitates*.

This powder has the following properties.

Its properties.

It is white and crystalline; but the size and lustre of the crystals are variable. Light alters it a little. Heat, a blow, or long continued friction, causes it to inflame with a brisk detonation. Pressure alone, if it be not very powerful, has no effect on it. It likewise detonates by the electric spark. It is slightly soluble in water. It has a very strong metallic taste.

Action of sulphuric acid on it.

Concentrated sulphuric acid occasions it to take fire, and is thrown by it to a considerable distance. Dilute sulphuric acid appears to decompose it slowly.

Of muriatic.

Muriatic acid, whether concentrated or weak, decomposes it immediately, and forms with it muriate of silver. The quantity of muriate it produces indicates, that it contains about 0·71 of metallic silver. A pretty evident smell of

Smell of prussic acid.

* It would be superfluous to remind the chemist, that the mixture of alcohol with hot nitric acid is liable to occasion accidents, and that it is consequently prudent, to operate on small quantities.

prussic

prussic acid is perceived the first moment of the mixture, but I never could discover any traces of it.

Decomposed by nitric acid; Nitric acid decomposes it by the help of a boiling heat; and the products are nitrate of silver, and nitrate of ammonia, if it be continued long enough.

sulphuretted hidrogen; It is decomposed by sulphuretted hidrogen; the ammonia and vegetable matter remaining in the liquor.

and potash. Caustic potash decomposes it; black oxide of silver being separated, and ammonia disengaged.

Ammonia dissolves it without alteration. It is soluble in ammonia; but by a slow evaporation it may be separated from it, retaining its original colour and other properties, particularly that of detonating by heat, and not by simple contact.

A virulent poison. Its most important property to be considered, with respect to the use made of it, is its action on the animal economy. Mr. Pajot-la-Forêt, who has made a great many experiments on this subject, has found, that very small doses are sufficient to destroy pretty strong animals, as cats. They all expired in the most dreadful convulsions. It is unquestionably one of the most violent poisons to be found among metallic compounds.

XVI.

Memoir on the Means of forming a Judgment of the Quality of Glass, particularly Window Glass, and distinguishing such as is liable to alteration: by Mr. GUYTON. *Read at the General Meeting of the Society of Encouragement, March the 11th,* 1807*.

Prize proposed for a test of good glass. ABOUT two years ago Mr. Guyton suggested to the society, to propose as the subject of a prize, a ready method of ascertaining the goodness of window glass. It appears, that, from the negligence or ignorance of the glass manufacturers, the windows in several large houses had become disfigured in a few months by a spontaneous alteration of the glass, which destroys its transparency: accordingly it was of some importance, to be able to guard against this inconvenience. The prize was proposed, and its term prolonged;

French window glass liable to spoil.

The question

* Abridged from Annales de Chimie, vol. lxii, p. 5, April, 1807.

but

but no paper was sent on the subject. This led Mr. Guyton to investigate it himself. unanswered.

With respect to the general characters of glass he observes, the specific gravity of different kinds of glass, all of which may be good, is very various; beside which, to ascertain it requires nice instruments, and an expertness in their use not commonly to be met with. Specific gravity.

The inspection of the fracture affords but a loose conjecture to the most experienced eye: though Mr. Ducloseau asserts, that the fracture of good glass is always wavy, and its angles more or less acute. Fracture.

The degree of hardness varies not only in different kinds of glass, but in glass of the same kind. Workmen used to handle the diamond readily distinguish glass that cuts soft, from that which cuts hard; so that this might seem a sufficient indication of its good or bad quality. It is very difficult however to discriminate degrees of difference in this respect, neither is it always to be depended upon. Hardness.

Glasses that are called greasy [*gras*] are bad insulators of electricity, and little capable of being rendered electric by friction. Probably this, as well as their being liable to alter, depends on an excess of saline flux, or an imperfect refining, that has left sandiver in them. Bad glass not a perfect non-conductor of electricity;

Bad glass placed on burning coals becomes dull and more or less meally on its surface. The same effect is more speedily and evidently produced before the blowpipe. But in all such trials we have to guard against the too rapid or too unequal action of the heat. and tarnished by heat.

The experiments Mr. Guyton made with neutral salts convinced him, that this mode of proof, which would have been very convenient, was altogether inefficacious. Among the specimens of the commonest window glass however, which is in general unalterable, he found one, that underwent a perceptible change on its surface by merely boiling in a solution of alum and muriate of soda. Neutral salts do not act on glass.

There are few glasses, which, when reduced to an impalpable powder, are not slightly acted upon by concentrated acids: though previously all well made glass resists sulphuric acid, the most powerful of all, even assisted by heat. On the contrary it very readily attacks glass of bad quality, at the Acids act on powdered glass

Bad glass corroded by sulphuric acid.

the common temperature. I have seen, says Mr. Guyton, black glass bottles, into which concentrated sulphuric acid had been put, perforated with several holes in the course of a few days; which, being largest interiorly, exhibited there white, silky excrescences, formed of the acid united with the soluble earths of the glass. But all glass susceptible of alteration in the air is not of so defective a composition; so that to form a judgment of it, the acid must be digested upon it, and the heat carried so far as to raise the acid in vapour. When this is done, it will leave untouched only well made and well refined glass, whatever be its nature, transparency, or colour.

This a good test.

Sulphuric acid then fulfils all the conditions required for a test. I foresee however, that this chemical instrument will frequently be neglected, from fear of the accidents, to which they may be exposed, who are not in the habit of using it with caution. This has led me to substitute an agent equally powerful, that may be procured more easily, and employed without any danger.

But may be neglected from fear.

This agent is sulphate of iron, the green copperas of the shops. After having put into a small Hessian crucible, or any other of stoneware, slips of the glass to be examined, either alone, or comparitively with others, let the crucible be nearly filled with this sulphate coarsely powdered. It may then be set on the fire, and kept there till the metallic salt has acquired a red colour. When the glass is grown cold, it only requires to be immersed in water, which will show whether it will be altered, and in what degree.

Easy mode of ascertaining its goodness by sulphate of iron.

This process is not expensive, requires no apparatus, and is in every one's power: and the results I have obtained appear to me to leave no doubt of its advantage.

XVII.

Report on a Paper on Nitrous Ether, read to the Institute the 4th of August, 1806, *by Mr.* THENARD, *Professor in the College of France. By Messrs.* GUYTON, VAUQUELIN, *and* BERTHOLLET*.

Various ethers.

VARIOUS kinds of ether have been formed by the action of some of the acids on alcohol. Volatility, inflamma-

* Annales de Chimie, vol. lxi, p. 282, March, 1807.

bility,

bility, and a peculiar smell, give to them all a common character, which does not allow us to confound them with other substances: yet we have only an imperfect knowledge of the differences that distinguish them, and our theory of their production is perhaps still more imperfect. It is true Messrs. Fourcroy and Vauquelin have thrown much light on the production of sulphuric ether; but their explanation does not admit of being extended to some others. It was of importance therefore to resume this subject, and treat it in a general manner. This Mr. Thenard has undertaken. In the first paper he has presented to the Institute he treats on nitric ether; from this he intends to proceed to others; and he will examine why some acids have the property of producing ether, and others have not.

Have a common character.

Yet differ.

The processes recommended by different chemists for the production of nitric ether, which Mr. Thenard reviews, are very discordant, and have no other object than the ethereal liquor to be obtained, without any analysis of the gaseous products, or any consideration of the circumstances of the process; if a memoir by the Dutch chemists be excepted, which Mr. Thenard examines particularly at the conclusion of his own. Accordingly he found himself obliged to treat his subject independantly of the labours of others.

Various processes of making it.

Mr. Thenard began with distilling a mixture of equal weights of alcohol and nitric acid, both of given strengths, in an apparatus adapted for the separation of the fluid from the gaseous products. A gentle heat is sufficient; and indeed the action soon becomes so brisk, that it is necessary to suppress even that. He afterward examined the residuum in the retort, the fluid product, and the gasses.

Equal weigh of nitric acid and alcohol distilled.

The residuum was composed of nitrous acid, acetic acid, alcohol, water, and a small quantity of matter, the nature of which is uncertain, but which is easily carbonized. The proportions of these he ascertained by ingenious and accurate methods. If the distillation be carried on to dryness, the viscous residuum contains oxalic acid, and probably malic.

Residuum.

The distilled liquor, which is considered in the shops as nitric ether, is composed of water, nitrous acid, acetic acid, ether, and probably alcohol.

The distilled liquor, or impure ether.

The gaseous product in particular required much patience

Gasses produced.

and ability, to separate it into its different elements, to assign to each of these the properties dependant on it, and to explain the difierences that result from the circumstances in which they are placed. It was composed of nitrous gas, azote, oxide of azote, nitrous acid gas, carbonic acid, and ethereous gas, which it was particularly necessary to separate from the rest, in order to examine its properties. By these experiments, which may be termed preliminary, the author was led to the following process for separating the pure ether, and examining it, either in the liquid or gaseous state.

Mr. Thenard's process.

Into a retort he put five hectogrammes [16oz. 48grs. troy] of alcohol, and as much nitric acid, and adapted to it in succession by means of glass tubes five tall bottles half filled with a saturate solution of muriate of soda. In the last was a tube opening under a jar to collect the gaseous part. All the bottles were surrounded with a mixture of pounded ice and salt, which was stirred occasionally. To commence the operation a little fire was applied, but it was soon necessary to extinguish it, and even to cool the retort.

The fluid left in the retort was similar to that of the first experiment already mentioned.

The ether produced.

On the surface of the solution in all the bottles was found a yellowish liquid; which weighed, when the whole was collected together, 255 gr. [8 oz. 98 grs]. That in the first bottle was a mixture of alcohol, ether, acetic acid, and nitrous acid: that in the others was nitric ether free from alcohol. The nitric ether in this state has a strong smell: it is specifically lighter than water, and heavier than alcohol: dissolves in any proportion in the latter, but requires near 48 parts of water to dissolve it, and at the same time is partly decomposed by it, as will appear below. It possesses the property of combustibility in a high degree; yet it strongly reddens infusion of litmus, owing to a little nitrous and acetic acid that it retains, and which may be separated by means of lime.

Its properties.

Much more volatile than sulphuric.

The volatility of ether thus prepared is so great, that it indicates a tension of 0·73 of a metre, while that of the best sulphuric ether, under the same circumstanes, is but 0·46 of a metre, at 21° of the centigrade thermometer [70° F], and at 0·76 of a metre [29·7 in.] of atmospheric pressure. At this

this temperature, and this pressure, therefore, it is at the limit of its existence in the liquid state.

But if nitric ether be deprived of its acidity by means of lime, it soon becomes acid again, whether it be redistilled, left in contact with the air, or kept in full and closed bottles. This formation of acid takes place, when ether is treated with water, particularly at a temperature from 25° to 28° [77° to 86°]. The author explains the formation of this acid by the reciprocal action of the principles that constitute ether, and which are found to be feebly retained in it by combination. Soon becomes acid.

Mr. Thenard next proceeds to the decomposition of nitric ether by heat, and analyses the gasses arising from it. Analysed by heat.

Founding his calculations on the most accurate data we possess, the result is, that 100 parts of nitric ether, rejecting fractions, are composed of azote 16, carbon 39, oxigen 34, hidrogen 9. Its component parts.

Hence he infers what passes in the reciprocal action of alcohol and nitric acid. The oxigen of this acid combines with a great part of the hidrogen of the alcohol, and with a very small quantity of its carbon. Hence result, 1st, a great deal of water, a great deal of gaseous oxide of azote, a little carbonic acid, a little nitrous acid and nitrous gas: 2dly, the separation of a small quantity of azote, and the formation of a great deal of nitric ether, by the combination of a pretty large quantity of the two principles of nitric acid with the alcohol dishidrogenized and slightly decarbonized: 3dly, the formation of a little acetic acid, and a small quantity of a matter easily carbonized, by the combination of a part of the hidrogen with carbon and oxigen. Action of nitric acid on alcohol explained.

Supported by these deductions, Mr. Thenard discusses the processes published before him for obtaining nitric ether, and he shows, that some are dangerous to attempt; that none of them furnish the whole of the ether, that might be obtained from the same ingredients; and that all of them yield only more or less compounded liquors, in which the nitric ether, though they bear its name, constitutes but a part. Former processes, some dangerous, all wasteful, and the ether impure.

The Dutch chemists have published some interesting researches on nitric ether, or rather on the gasses obtained by the action of nitric acid on alcohol. But to explain the curious The hypothesis of the Dutch chemists erroneous.

curious facts they have made known, they have employed an insufficient hypothesis. 1st, They have considered the gas in question as a compound of nitrous gas and ether; while it is composed of gaseous ether, nitrous gas, nitrous acid, azote, oxide of azote, carbonic acid, and acetic acid, in short of all the substances capable of assuming the gaseous state, in the various circumstances under which they are found. 2dly, They have supposed ether to be a substance always identically the same, so that they neglected to analyse nitric ether, and establish its peculiar characters. 3dly, In consequence of this opinion they have been led to ascribe to a preexisting nitrous gas phenomena, that are owing to a decomposition of nitric ether.

If the process were varied, the effects would differ;

After having discussed the opinion and experiments of the Dutch chemists, Mr. Thenard concludes his paper by observing, that he has considered only the products and phenomena obtained by given proportions, and under certain circumstances. The effects must be different, when these are varied; and he intends to subject them to experiment:

but that above given the best.

but he has already satisfied himself, that those he has employed are most favourable to the production of nitric ether.

The memoirs printed by the Institute.

The committee concluded, that the memoir at large merited insertion in the Collection of foreign Papers, and the class adapted their conclusion.

XVIII.

Observations on subterranean Heat, made in the Mines of Poullaouen and Huelgoat in Brittany: by J. F. Daubuisson*.

Facts respecting the interior heat of the earth wanting

THERE are few questions in natural philosophy, respecting which we are more in want of positive and well established facts, to deduce consequences from, than that respecting the temperature of the interior part of our globe, taken at depths we are able to reach. I have already made known some facts I observed on this subject in the Saxon mines,

* Journal des Mines, February, 1807, p. 119.

and

and I shall now give some others noticed last summer in Brittany. The habit of making similar experiments, and the knowledge I had of the places, enabled me to choose with some discrimination the points of which I ascertained the temperature; so that I trust the facts I have recorded will not be uninteresting to those, who make our Earth an object of their study.

Thermometer used.

The thermometer I employed was of mercury, and divided into eighty degrees from the freezing to the boiling point of water. It was enclosed in a tube. I found by trial, that when it indicated a given temperature, and was made to deviate from this about a dozen degrees, it required three or four minutes to bring it back to the former point by immersing it in water of that temperature, and eleven or twelve minutes if kept in the open air. Hence, whenever I was desirous of ascertaining the temperature of a body of water in the mines, I immersed the thermometer in it entirely, and left it there five minutes; and when I took the temperature in the air, I let it remain a quarter of an hour. All the observations were afterward reduced to degrees of the centigrade thermometer. Notwithstanding all the care and patience I employed however, I cannot answer for their exactness to less than a quarter of a degree.

3′ or 4′ to alter 21° F. in water;

11′ or 12′ in the air.

Observations made at Poullaouen.

Situation and topography of Poullaouen.

THE mine of Poullaouen is in latitude 48° 17′ 49″ N., and longitude 5° 55′ 57″ West of Paris. Its mouth, that of St. George's pit, is 106 met. [347½ feet] above the level of the sea. It is 4 myriam. [25 miles] from the seacoast of Brittany on the north, and 6 [37½ miles] from that on the south and that on the west. It is in that tongue of land, which advances into the ocean under the form of a roof, raised in its centre about 260 met. [853 feet] above the level of the sea, and constitutes Brittanny. The country round the mine, to the distance of near 6 miles, is about 150 met. [490 feet] above the level of the sea; and is intersected in every direction by valleys, one of which is an almost circular basin about a millimetre [1093 yards] in diameter, that forms the roof of the mine.

According

Mean temperature by calculation.

According to the law of the temperature from the equator to the pole, the mean temperature of the surface at Poullaouen should be 12·4° [52·5° F.]*. The elevation of the soil requires near 1° [1·8° of diminution, so that the mean temperature may be estimated at 11·5° [50·9° F.]

Observations made 5th of Sept. Day fine.

My observations were made the 5th of September, 1806. During the whole day the weather was fine, and but few clouds were seen. The temperature in the shade, in the middle of the day, was 19° [64·4° F.]. In reporting my observations I shall mention the situation of the places where they were made, as well as whatever appeared to me capable of influencing the temperature. Opposite each expression of the temperature I shall note the depth of the place below the surface of the ground.

Tabulated temperatures and depths, with remarks.

	Temperature by Fahrenh thermo.	Depth English Ft. In.
1. In the first gallery, called *fifty foot level*, near the shaft, in a place where there was but a slight current of air, a little water that lay on the ground indicated	53·8°	52··3
2. In St. George's gallery, under the intersection of three branches of the vein, in a kind of cul de sac, very remote from the places where the miners were at work, in which there was no current of air, but a large quantity of water filtered from the roof: this water was ..	51·6°	127··4
3. The water that thus filtered into the gallery, when it reached the well from which it was raised, was	52°	127··4
4. Thirty six metres lower, at the level of Boullaye, toward the end of a long gallery, where there was no current of air, and no person at work, under very strong percolations and in the water I had	51·6°	244.10

* Theory and observation have led me to a very simple expression of the thermometrical temperature of a place, the latitude of which is known. This expression is 30·7° [85·4° F.]. cos. $^{2}\cdot^{2}5$ latitude; or with sufficient exactness in the temperate zone 28° [80·6° F.]. cos. $^{3}/_{2}$ lat.

† In the atmosphere the temperature diminishes 1° [1·8° F.] for every 175 met. [191 yards] in height.

5. At

	Temperature.	Depth.	Tabulated temperatures and depths, with remarks.
5. At the bottom of St. George's shaft, in the well in which the waters that penetrate into the lower parts of the mine around it are collected	55·7°	463..6	
6. The air over this water	57·2°	469..3	
7. In the well at the bottom of St. Barbe's shaft, at the other extremity of the mine	54·5°	489..6	
8. In the air above this water	56·1°	489..6	
9. The waters of the old excavations, that run into this well	54·1°		
N. B. These waters arising from filtrations that take place chiefly in the upper parts of the old workings are cold; and as they form the greater part of those that enter into St. Barbe's well, they are the occasion of the lowness of the temperature shown by the water in it.			
10. In an excavation but little distant from the bottom of St. Barbe's shaft, called the oven gallery, the sides of which are almost every where interspersed with radiated pyrites partly effloresced, the thermometer left a quarter of an hour in a small hollow made in the midst of the pyrites, and which contained a great deal of white sulphate, indicated	56·5°	457	
11. Afterward put into a small hole, from which a pretty strong spring issued, it equally marked	56·5°	457	

The observations 2, 3, and 4, incontestably prove, that the heat of the rock in the upper parts of the mine is 51·8°. The waters indicating this certainly acquired the temperature of the rock in filtering through it; and this temperature does not differ in any sensible degree from that indicated by theory. If the first observation showed a little higher temperature, it is because it was made in a place, where air from without, and consequently warm, as the experiments were made at the end of summer, was continually passing. General deductions. Superficial temperature agreeable to theory.

Observations 5 and 6 show, that the temperature of the lower Temperature

increases with the depth.

lower parts of the mine is more considerable than that of the upper parts. If in the depths the air appear hotter than the water, it is probably because it has retained a part of the heat it possessed when it entered the mine. I have already pointed out the reason why in 7, 8, and 9, the heat was less than the depth requires.

Pyrites do not always occasion heat.

Experiments 10 and 11 show, that there are circumstances in which the presence of pyrites does not occasion heat. That which they indicated was not occasioned by them, for in St. George's shaft, where there was none, the temperature was the same.

Heat increases 4° or 5° in 100 yds. of depth.

Thus, setting aside every extraordinary cause, the observations I have reported appear to me to indicate, that at the depth of 150 met. [163 yards] the temperature at Poullaouen is 2° or 3° [3·6° or 5·4° F.] more than at the surface.

Observations made at Huelgoat.

Huelgoat mine.

THE mine of Huel is in latitude 48° 18′ 17″ N., long. 6° 1′ 46″. The opening of its shaft is 173 met. [188 yards] above the level of the sea; and is on a large hill, that separates two valleys, the depth of which is 80 or 90 met. [87 or 97 yards].

Mean temperature by calculation.

From this latitude and elevation we may infer the mean temperature to be 11° [50° F.]

In schist.

The rock, like that of Poullaouen, is an argillaceous schist, but it likewise contains some strata of aluminous schist.

Observations the same day.

The following are the observations I made on the 5th of September, the same day as those at Poullaouen.

Tabulated temperatures and depths, with remarks.

1. In a gallery about 16 yards below that by which the workmen commonly enter the mine, into which no person had been for several years, which has no other outlet, and in which there was no current of air, a thermometer at its northern end after twenty minutes indicated 50°

Temperature by Fahrenh. thermo.	Depth English Ft. In.

After having descended the shaft called the miners, and gone a few dozen yards into the

gallery

gallery at its foot, I entered another shaft, which terminates in a gallery that has no communication with the rest of the mine, and in which consequently there is no current of air.

Tempe-rature. Depth. Tabulated temperatures and depths, with remarks.

2. Here the thermometer immersed in a little stagnant water on the ground rose to 52·2° 228..6

3. I then ascended to the former gallery, and under a strong infiltration, in the water, and in a place traversed by a current of air, the thermometer indicated..... 54·9° 195.10

I then directed my course to the south, where the present workings are.

4. At the second gallery, at a little distance from the main shaft, in a place where people are continually passing, and where there was a pretty strong current of air, a little stagnant water indicated..... 57·2° 261

5. At the fifth gallery, the thermometer immersed in a reservoir of water, which was near the main shaft, rose to 60·8° 457

6. At the extremity of the gallery, No. 9½, the part where the works are farthest advanced in a southerly direction, a large quantity of water, slightly vitriolic, spouts from the rock. The thermometer, kept a quarter of an hour in the midst of this stream, constantly marked..... 65·7° 750

7. Held on one side of it, in the open air, it equally gave the same temperature 65·7° 750

8. It was the same about sixty paces toward the shaft, when immersed in the middle of the rivulet formed by the spring just mentioned 65·7° 750

The bottom of the mine was inundated, the water lying on it about 16 met. [52 feet] deep: and by a small shaft, at a little distance from the main shaft, I descended to the level of this subterranean lake.

9. The thermometer, kept a quarter of an

hour

Tabulated temperatures and depths, with remarks.

	Temperature.	Depth.
hour on a board floating on the water, indicated	64°	776
10. Immersed in the water it equally indicated	64°	776

All the water that enters into this southern part of the mine runs into the lake, from which it is pumped up.

11. The temperature of the water issuing from the pumps into the gallery No. 7 was	65·1°	587·6

Following the course of this gallery the water runs to another shaft in the north part of the mine.

12. There it mixes with a small quantity of water, the temperature of which is	57·2°	391·8
13. And when the water thus mixed is conveyed by the pumps to the discharging gallery it raises the thermometer to	63·3°	

General deductions.

We have here two classes of observations to distinguish; those made in the north part of the mine, 1, 2, 3, and 12; and those made in the south part.

Natural temperature

The first appear to me to indicate the natural temperature of the soil. No. 1, made 20 or 30 yards below the surface of the ground over that part of the mine, must be considered as giving the true degree of heat of the surface of the earth in that country. I perceive no cause, that can have altered the natural temperature of the place, which is very far from all the workings: certainly it continues the same throughout all seasons: and its expression is precisely the same as theory indicates. Observations 2 and 3 show, that this temperature increases, as we penetrate deeper. The current of air in the first gallery, that through which the ore is conveyed in wheel barrows, accounts for the small excess of heat observed in it proportionally to the depth.

agrees with the theory.

Increases with the depth.

Increased in one part by thermal waters.

As to the temperature of the observations made in the south part of the mine, it is visibly influenced by a foreign cause, the arrival of vitriolic waters coming from the south. On sinking a new shaft a hundred yards from the southern part of the present workings, strata of aluminous schist were traversed, which had a very strong styptic taste as soon

Cause of their heat.

as extracted. With a lens a number of pyritous points may be observed in it, which by their decomposition and action on the schist probably produce an evolution of caloric, that heats the water traversing these strata. These being at no great depth may have a communication with the atmosphere through some fissures, and thus the decomposition may have been effected.

However this may be, it appears to me certain, that the water must have acquired the heat of 20° [66·2 F.], which is much more than naturally belongs to their depth, by traversing these strata.

Pyrites in connexion with schist.

I shall here observe too, that, if the heat is to be ascribed to the pyrites, they produce it by their action on the schist. In the observations made at Poullaouen, we found a considerable quantity of pyrites, without any particular heat being produced: and I must repeat what I have said elsewhere, that I have seen pyrites extracted, and found the heat not perceptibly greater than in other mines. Accordingly I am inclined to believe, that pyrites in the mass, or at least not radiated, do not produce subterranean heat: but such as are disseminated in extremely small parts through bodies on which sulphuric acid is capable of acting are in a different case, when the atmospheric air can have access to them. In another paper I remarked, that the inflammable air called fire-damp in coal-mines does not proceed from the coal that contains most pyrites, but from that in which scarcely any is discernible, and where the sulphuret of iron is probably in imperceptible particles.

Pyrites do not always occasion heat.

Firedamp not produced by the most pyritous coal.

XIX.

Letter from Dr. Veau-de-Launay *to J. C. Delametherie, on the Production of oxigenized muriatic Acid by the galvanic Pile*[*].

THE experiments with respect to the production of the oxigenized muriatic acid, obtained in distilled water by the

Acid obtained by galvanism.

[*] Journal de Physique, Vol. LXIII, p. 472, Dec. 1806.

galvanic

in small quantities hitherto.

galvanic action, have hitherto exhibited but a small quantity of acid; which has led persons to question, and even to deny, the result of these experiments. What has lately been done by the experimental class of the Galvanic Society will leave no doubt with regard to the product. Fifty grammes, or nearly an ounce and half of distilled water, have passed into the state of oxigenized muriatic acid, and dissolved a gold wire of the length of about 0·07 of a metre, or 2¾ inches [2 in. 7 lines Eng.], immersed in the cylinder, containing the distilled water, which had acquired a very sensible and even strong smell of that acid, as well as a yellow colour, such as a solution of gold exhibits. To these different characters, easy to distinguish, were added those of the action of different reagents. The tincture of litmus was reddened powerfully; the solution of nitrate of silver was speedily changed, and a precipitate of muriate of silver thrown down.

1½oz. of water converted into oxigenized muriatic acid, dissolved 2·7 in. of gold wire.

Precipitated by nitrate of silver.

I can assure you, there is not the least doubt of the certainty of the results displayed by this conversion of distilled water into oxigenized muriatic acid.

If the acid were extricated from the salt merely, still interesting.

Even if the result of the experiment were to be considered as an extrication of the muriatic acid from the solution of salt contained in the cylinder of the copper pole, a very interesting phenomenon would remain to be considered in this product. This experiment therefore deserves particular attention; and it is to be wished, that it was varied and repeated in different ways. I doubt not but the result will offer facts of considerable importance to the science of natural philosophy.

SCIENTIFIC NEWS.

New Bavarian Academy of Sciences.

THE Bavarian Academy of Sciences at Munich, according to its new constitution, is to have a more extensive field for its labours than any other in Europe. Under the direction of the ministry, it is to have the immediate superintendance of all the public seminaries of education in Bavaria, from the universities down to the primary schools. It will be composed of learned natives, and foreigners of celebrity invited by the government from other parts of Germany.

many. Privy counsellor Jacobi is still talked of as its president. Among the other academicians, whose names have been mentioned, are Mr. Seyffer, whom the emperor Napoleon appointed engineer geographer in the war against Russia and Austria, and who has been director of the observatory at Gottingen; Eichhorn, the historian and orientalist, whom the king has likewise called from Gottingen; Wiebeking, of Vienna, whose skill in hydraulics has already been of service to the kingdom; and Wolf, known by a very good history of the Jesuits, to whom all the archives of Bavaria are opened, for the purpose of his compiling a national history.

Royal library at Munich.

The royal library, which is already a very good one, will be increased by a committee appointed to select for it every thing of value in the libraries that are suppressed.

Collection of paintings.

The collection of paintings at Munich has long been celebrated; but, since the galleries of Manheim and Dusseldorf have been added to it, it is unquestionably the finest in Europe, next to the Napoleon Museum.

Royal Academy of History & Antiquities at Naples.

A decree has been issued at Naples for forming a society consisting of forty men of letters, to be called the Royal Academy of History and Antiquities. The first twenty members are to be named by the king; and when these have assembled, they are to nominate three persons for each of the remaining vacancies, out of whom the king will choose one to fill it. It is to have a perpetual secretary appointed by the king, and to choose its own president for three months. The directors of the museum, excavations, and royal printing office, are always to be chosen from its members. The academicians are to be admitted at court.

Mechanical imitation of various wind instruments and others.

Mr. John Maeltz, of Vienna, has exhibited at Paris a musical machine of his invention, to which he has given the name of *panharmonicon*. This machine, moved entirely by springs, gives the sounds of various wind instruments with a clearness and perfection never before attained. The instruments that compose it are the german flute, flagelet, clarinet, hautboy, bassoon, horn, trombone, serpent, and trumpet; beside kettle drums, a great drum, cymbals, a triangle, &c. Pieces are performed by it with great precision, and the forte and piano distinctly marked. The execution

cution of the trumpet is particularly astonishing. For each instrument the inventor has contrived a mouthpiece adapted to its nature, which answers with the greatest perfection to the capacity of the human organs.

Intended tour in the East.

The chev. von Hoegemuller, superintendant of the Austrian military studs, is to set off in November on a tour in the East, with the necessary instruments and attendants, under the patronage of prince Charles. His principal object is the natural history of the horse; but he will make a point of endeavouring to answer any questions, that shall be addressed to him by the learned who cultivate geography, philology, archæology, numismatics, &c. He intends to traverse Hungary, Transylvania, Buchowina, the Ukraine, embark at Odessa for Constantinople, and thence proceed to Aleppo.

Ancient busts made by American Indians.

Mr. Jefferson, the president of the United States of America, has in possession several busts made by Indians. They are nearly of the natural size, and reach to the middle of the body. The features are well marked, and characteristic of the copper-coloured or American race. In one, representing an aged savage, the wrinkles and the expression of the countenance are very well marked. These busts were found in digging at Palmyra, on the river Tennessee. The substance of which they are formed, and which is extremely hard, is not known: some suppose them to have been cut by the chissel out of solid stone; others that they are a composition, first moulded, and then burned. Whether they were idols, or busts of distinguished persons, is equally questioned. Who were the progenitors of the present race of Indians, that were capable of thus executing a tolerably good resemblance of the human head, face, neck, and shoulders?

Extensive ridges of shells in America.

General David Merriweather writes to Dr. Mitchill of New York, that the vast banks of shells commencing on the southern bank of the Savannah, near White Bluff, extend in a right line through a space of about a hundred miles from the borders of the sea toward the south-west. The ridges are not entire, but the ground is more elevated to the breadth of six or eight miles than it is above or below. Not only oyster-shells, but those of cockles, and others,

are

are found. Some are entire, and very large: others are agglutinated as by a cement. Some are large enough to contain a man's foot. In different parts of the eminences for forty miles some of these shells occur. They are used for making lime; but a little higher up to the south-west there is a heap of shells forming a kind of rock, that is preferred for this purpose. At some distance still higher, and in the same direction, there are several quarries of a kind of siliceous stone, in which a great number of shells of all kinds are interspersed here and there. These are petrified, and as hard as the flint itself. Millstones are made of it, in quality nearly the same as those of France.

Fossil shells in silex,

hard as flint.

Millstones made of it.

At Hudson's Bay some experiments have been made with frozen mercury. It was reduced to a plate as thin as paper, by beating it on an anvil with a hammer brought to the same temperature as the mercury. A piece of it being thrown into a glass of hot water, the water froze instantly, the glass flew to pieces, and the mercury became fluid.

Quicksilver beaten into a thin plate.

Dr. Baconio of Milan has lately composed a galvanic pile entirely of vegetable substances. He forms it of disks of red beet root, two inches in diameter; and disks of walnut tree, of the same size, divested of their resinous principle by digestion in a solution of cream of tartar in vinegar. With this pile he produces galvanic effects on a frog, taking a leaf of scurvy-grass for an exciter.

THE seventh number of the new series of the Mathematical Repository, by Mr. Thomas Leybourn, contains: 1. Solutions to thirty curious mathematical questions proposed in a former number; 2. Solutions to some mechanical problems by Mr. John Dawson; 3. Solution of a curious diophantine problem by Mr. Cunliffe; 4. An essay on the theory of amicable numbers by Mr. John Gough; 5. An investigation of some theorems for finding the sums of certain infinite series by Mr. Cunliffe: 6. Le Gendre on elliptic transcendentals: and 7. Thirty new questions to be answered in a subsequent number.

Mathematical Repository.

Mr.

New mineralogical work.

Mr. ACCUM, to whom the public is indebted for a System of practical Chemistry and several other works, has put to the press a System of Mineralogy and Mineralogical Chemistry, and its application to the arts. This work, which is formed chiefly on the plans of Haüy and Brongniart, will be in 2 vols. 8vo. with eight copperplates.

Lectures on Surgery, and on Physiology.

Surgical and physiological lectures.

Mr. A. CARLISLE, F.R.S. F.L.S. and surgeon to the Westminster Hospital; will begin his course of lectures on the art and practice of surgery, in all its branches, on Tuesday, October 6th, at eight o'clock, P. M. at his house in Soho Square. The subject will be continued on Tuesdays, Thursdays, and Saturdays, at the same hour. The diseases and accidents allotted to the province of surgery will be fully treated of, and illustrated by cases from the lecturer's experience. The different operations will be demonstrated, and the anatomy of the parts explained. On the same evenings, a course of lectures will be delivered on the natural history, physiology, and pathology of the human body, calculated to illustrate the several processes of healing, and to afford a compendious view of the animal economy. The introductory discourse will be open to all students.

Medical and Chemical Lectures, St. George's Hospital, and George Street, Hanover Square.

Medical and chemical lectures.

ON Monday, October 5th, a course of lectures on physic and chemistry will recommence at No. 9, George Street, Hanover Square, at the usual morning hours: viz. the medical lectures at 8, and the chemical at 9 o'Clock. By George Pearson, M. D. F. R. S. senior physician of St. George's Hospital, of the College of Physicians, &c.

Note, a register is kept of the cases of Dr. Pearson's patients in St. George's Hospital, and an account is given of them at a chemical lecture every Saturday morning at 9 o'clock. Proposals may be had at the Hospital, and at No. 9, George Street.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1807.

ARTICLE I.

Facts toward a History of Pitcoal: by PROFESSOR PROUST*.

A Coal of Decise, distilled by Sage, left ·59 or ·60 of carbonaceous residuum, or coak. A coal of Cévènes yielded Berthollet ·76 or ·77; and that of a pit in the forest of Gensane ·75. I obtained from a coal from England ·64: from Lieres ·60, and from Fondon, both in Asturia, ·64: from Belmez, in Estremadura, ·65: from Villa Nueva near Seville ·68: from Quiros ·70, las Camaras ·70, Langreo ·75, la Rionda ·76, and la Riosa ·77, all in Asturia. Coal left from 59 to ·77 of coak.

The environs of Madrid exhibit a few vestiges of earth impregnated with bitumen, but no coal, as was hoped; for there is not a city in Europe at present so much in want of it. I have visited no coal pits in Spain, but some of those of the fertile and picturesque province of Asturia; a country that exhibits in miniature whatever of grand and sublime the traveller admires in the Alps. The coal there in general is in veins of little thickness; scarcely any so much as 18 inches. No coal near Madrid, where it is much wanted.

The following were the products of a hundred pounds of some of the coals I examined.

* Abridged from Journal de Physique, vol. LXIII, p. 320, Oct. 1806.

Products of different coals.

	Coak.			*Oil.*			*Water.*			*Gasses lost.*		
	lbs.	oz.	dr.	lbs.	oz.	dr.	lbs.	oz	dr.	lbs.	oz.	dr.
Villanueva	68	8	4	7	9	0	4	8	6	19	5	6
Belmez	70	0	2	10	4	0	7	7	2	12	7	4
Langreo	75	0	0	11	11	4	4	7	2	8	3	2
English	64	1	0	7	0	4	12	14	2	16	0	2

General conclusions.

From these observations it follows, that the oily product is more abundant than the aqueous from three of these coals.

2. That the weight of the gasses is as variable as that of the liquid.

3. That the real quantity of coal furnished by these bituminous substances is in general above ·60, and less than ·80.

4. That it is three times as much as wood affords: and we know nothing but indigo, that can be compared with pitcoal in this respect.

5. That the matter of pitcoal varies as much in its carbonaceous, oily, gaseous, and other products, as the organized bodies, that are formed at present before our eyes.

6. That its oily produce is in general much greater, than our resinous woods, as the oak, elm, ash, &c., can furnish.

7. That pitcoal is thrice as serviceable, in furnaces that admit its use, as any wood known, since it leaves thrice as much coally matter.

8. That its coak, in consequence of its azotization, derives from our atmosphere much more fire than charred wood, since it cannot burn but by decomposing a much larger quantity of oxigen,

9. That as the oily and gaseous products are formed as well in the open air as in close vessels, some coals produce more flame than others, and are consequently better adapted to furnaces where a considerable current of flame is required; as those of bronze, porcelain, earthenware, &c.

10. That those which leave most coak after distillation will consequently last longer in iron works, reverberatory furnaces, &c.

Those which yield most tar not easiest to distil.

Of these four coals perhaps the richest in oil would not be the most easy to distil, at least by lord Dundonald's method, since they run, swell up, and agglutinate, so as toward

ward the end to become a uniform mass, not to be divided without labour.

All pitcoals in general give out more or less sulphurous acid toward the end of their combustion, which is owing to pyrites. At first I supposed, that they contained a peculiar compound of sulphur and carbon; but all that I passed through nitric acid, to free them from pyrites, burned to the end without emitting the slightest smell of sulphurous acid. Yield sulphurous acid.

The oily product of these coals varies greatly in consistence: it is more or less fluid, and a direct experiment is always necessary, to ascertain how much thick oil, or tar, it will afford. Is this tar really more preservative, and better adapted to retard the spontaneous oxidation of cordage, and the rigging of ships, than that of resinous wood? This has been asserted, but should it not be farther examined? Oily product differs in consistence. Should be farther examined.

The light oil separated from it is succinated, and not unpleasant. It is readily turned brown by the air. No particular use for it is known. Light oil.

The aqueous product contains carbonate of ammonia, but in small quantity. I did not find in it any vinegar, but I could wish to examine this again; particularly as I find in my notes, that I separated a little succinic acid from it by treating it with the muriatic. Water contains carbonate of ammonia, and succinic acid.

The gas is an oily hidrogen, that burns with a white flame, which limewater diminishes very little, because the ammonia retains almost all the carbonic acid. Gas, oily hidrogen, with very little carbonic acid.

Naturalists, reflecting on the similitudes that analysis perceives between the composition of vegetables and that of pitcoal, have pretty generally supposed, that coal may have been produced by heaps of fern, polypody, reeds, and aquatic plants destroyed, the impressions of which are retained by the adjacent strata; or even by trees, and their fragments, such as those which certain rivers by their inundations sweep away from the earth; and that the sea has broken them by its agitation, and accumulated them in the basins, from which we at present extract them. But the mechanism, and even possibility of these operations, simple as they are in appearance, are exposed to innumerable difficulties, when we examine them in detail. Coal supposed to be produced from vegetables.

Objections to this hypothesis.

For instance, the most elevated parts of the Globe on which depositions of coal have been found, as the Cordillera in Peru, where Leblond met with them, being more than four thousand yards above the level of the sea, do not easily bend to the explanation attempted, when it is alleged, that they are forests or plants swept away and comminuted by the waters. Had the sea at that period no lower place, on which to deposit its mud of broken plants? or was the Cordillera itself very favourable to the production of vegetables? These objections, which were made by Patrin, are not easy to remove. If we consider farther, that these immense tracts of carbonaceous mud, which resemble torrents of melted resin that a volcano has vomited out at once into valleys ten, fifteen, or twenty leagues long, and to the height of thirty, forty, or sixty feet, exhibit not the slightest interruption, not the least vestige of fishes, shells, bones, or stones, in their beds; no foreign body in their mass; to indicate those convulsions, or that disorder, which the imagination cannot easily separate from such great devastations of forests, mountains, and continents: we must confess, that such productions are not explicable by some of those accidents, of which nature at present gives us occasionally examples.

Alternation of their strata with sandstone.

Beside that the recurrence of fifty or sixty strata of coal, with as many of sandstone, do not allow the mind to conceive, how these two kinds of sediment can have been accumulated exclusively; as if previous to these periods the earth could produce nothing, but what furnished the sea with trees to pulverize, and silex to precipitate, alternately; and neither beast, nor bird, nor rock, nor flint, nor gravel, to disturb their continuity: we must likewise observe, that the coal, as we now find it, has certain characters, that will presently be mentioned, which perhaps place it at a greater distance from vegetable than from animal substances.

Impressions of plants in the surrounding schist only prove, that such then existed:

What data then have we after all for ascribing to plants the origin of bitumens? a few traces of mosses or ferns, scattered through the leaves of slate, that serve as their envelope? Such vestiges prove at most, that nature, during the period of these great operations, likewise made plants grow, and nourished animals in the seas, since we find shells in the strata that separate the coal in some countries; but not

not that it reaped from those plants sufficient materials to fill those inexhaustible lakes of bitumen, that interspèrse our globe from one pole to the other, and which the generations yet to be born will perhaps never exhaust. They would prove too, if this could be doubted, that it did not fabricate this composition, as it does that of minerals, in the interior of the earth, but on its surface only; that is, in the region it has chosen for the existence of organized beings.

and that coal originated on the surface of the Earth.

It is true, that trunks of trees are found in veins of coal: consequently trees existed at that period. But are these trunks themselves coal? Have they been analysed with a view to compare their products, and examine whether similar changes have taken place in these trees, and in those that are supposed to have been converted into coal? The importance of analysing fossil wood in this respect appears obvious. If it were once demonstrated, that a fossil trunk of a tree contains charcoal in the same proportion as the bitumens surrounding it; and that this charcoal, beside its degree of concentration, is combined with a fresh dose of nitrogen, so as to have lost that prompt and easy combustibility, which characterizes the charcoal of our woods; we might flatter ourselves, that we had an argument of great weight in favour of the opinion here attempted to be shaken: and we should be less surprised to find in this astonishing result of their metamorphosis, pitcoal, seventy or eighty per cent of charcoal; that is to say a proportion, which, if it had been that of the vegetables existing before those periods, would appear difficult to reconcile with that elastic and robust organization, which our forest trees require, to raise firm and secure trunk, and resists the storms of an atmosphere agitated like ours. This weak part of the grand problem may soon be elucidated, if our cabinets do not delay the eagerness of chemistry to decipher the medals of this kind they contain: and if natural history, assisted by the light of analysis, do not discover something more satisfactory, than any thing that has yet been advanced respecting the origin of pitcoal, we ought no longer to waste our time in reasoning on this prodigious event in geology, but banish all the learned hypotheses that have been started on the subject,

Trunks of trees found in it, are they really coal?

Analysis of fossil wood important to the question.

Quantity of charcoal in pitcoal inconsistent with such vegetables as ours.

ject, in consort with those romances concerning the origin of metallic veins, to which our age has given birth.

Coal as much resembles an animal as a vegetable product.

I have said, that analysis discovers in these bitumens characters, that do not show a greater affinity to vegetables than to animals. The following are the facts: and I leave it to the learned to decide which way the balance inclines.

Its smell not like that of wood, but friendly to the lungs.

1. The smell that coals exhale when heated is aromatic, succinated, and decidedly resinous: it irritates neither the eyes nor the lungs, like that of wood or vegetables when burned: and this resinous smell has even been considered as friendly to the diseases of the chest.

Softens and agglutinates by heat.

2. All coals soften, lose their shape, run, mould themselves to the shape of the retort, and fill it with a spongy or puffy coal, like that common to mucous substances, resins, indigo, gluten, and animal matters; but not like that of any known wood or plant.

Yields more oil and ammonia, but no vinegar.

3. Distillation obtains from it a lighter, more aromatic, and more abundant oil, than the nonresinous woods used for fuel; and a great deal of water and ammonia, but none of that vinegar, which abounds in the distillation of our vegetables; that empyreumatic acid, which renders their smoke so troublesome and suffocating; vinegar which is formed, whenever oxigen is an integrant part of an organic oxide, and the absence of which in the products of coal would authorise us in a certain degree to doubt, that oxigen is one of its products*.

Coak does not burn like charred wood;

4. The combustion of coak does not at all resemble that of our vegetable charcoals. It is slow and difficult like that of mineral coals, because it likewise contains condensed nitrogen; accordingly it requires a condensed atmosphere to burn it.

affords prussic lixivium;

5. Coak passed through potash always affords a prussic lixivium, which vegetable charcoals in general do not.

cannot be set on fire by nitric acid;

6. Animal charcoal cannot be set on fire by nitric acid, even after it is disazotized by potash: neither can coak, even passed through this alkali.

Pyroligneous acid forms acetate of copper.

* I for some time doubted the fact, that the pyroligneous acid was really vinegar; but I am now convinced of it, as the salt I formed with oxide of copper and the acid of elm displayed its characters after three purifications. It gave large rhombs, differing in no respect from acetate of copper.

A mixture

A mixture of nitre and coak burns with the same difficulty as mixtures of nitre and charcoal of blood, white of egg, indigo, &c. and mixed with nitre burns difficultly

7. There are few kinds of wood, that do not leave more ashes than pitcoal. Coal therefore is not clay impregnated with bitumen, as some naturalists have thought. Coal leaves less ashes than wood: therefore not clay mixed with bitumen.

I have not yet found any oxide of manganese in animal charcoal; and I have sought it equally in vain in the five kinds of coal mentioned above. No oxide of manganese.

8. Vegetable ashes contain a great deal of carbonate of lime, beside magnesia, alumine, and silex. Those of the five coals I examined afforded me only a great deal of silex, a little magnesia, alumine, and sulphate of lime, but very little carbonate; and in particular not an atom of those salts, which are habitually contained in our vegetable ashes; no phosphate, no muriate of soda, though the mud of these bitumens is supposed to have been formed with sea-water. Ashes of vegetables differ from those of coal.

10. All the soft or liquid parts of animals contain sulphur. They cannot be dissolved in potash, without having the solution loaded with it. Wool cannot be dipped in a bath of litharge and lime without being blackened, in consequence of the sulphuret of lead that adheres to it. Contains sulphur.

If no pitcoal be absolutely free from pyrites, is it not because the sulphur and iron, those two habitual elements of animal matters, have withdrawn from the organic substance converted into bitumen, to form a separate combination? None free from pyrites.

I content myself with bringing together these facts, and refrain from deducing any consequences from them, as they require to be compared with a greater number of coals, that it may be known whether they be as general as I suppose. But we now come to other properties, which separate still more the analogies supposed to exist between pitcoal and vegetables. Farther differences.

If, for example, the carbonaceous principle be an element of their composition, in the same manner, and in the same sense, as it is in our vegetables and animals, we shall show, that it is very feebly combined however, very weakly enchained at least by the hidrogen, nitrogen, and oxigen: I could almost venture to say independant of them, since it may be extracted from pitcoal by means, that certainly would never Charcoal feebly combined in coal.

never succeed with any vegetable or animal production we know.

Coal treated with nitric acid.

Let powdered coal, such as that of Villanueva, which affords 68 per cent of coak, be heated in nitric acid of 18° or 20°; and thus in a few moments it will be deprived of the property it had before of forming oil, oily gas, and ammonia. The coal thus prepared, washed, and dried, and then exposed to a moderate heat, affords products of a new order, but with indications of those just mentioned, and is reduced to ·66 or ·67 of coak.

reduced to coak.

This not the case even with indigo.

All pitcoals afford this extraordinary result, which is obtained from no organic production known, not even from indigo, though it contains ·72 or ·73 of charcoal, or much more than many kinds of pitcoal.

The same as produced in distillation.

The carbonaceous principle, freed from the other elements of the compound by this method, has all the properties of what would be obtained by the simple distillation of pitcoal; for, if it be treated with potash, it gives a prussic lixivium like coak itself. I proceed to the consequences.

Hence the blackness and brittleness of coal.

If in these bitumens the carbonaceous part be thus feebly enchained by the other elements, and consequently approach in its properties charcoal uncombined, we need not be surprised either at the blackness or fragility of pitcoal.

The charcoal in coal retains some hidrogen and nitrogen,

The carbonaceous principle of organized bodies, freed from the fetters of combination by any means, and consequently brought nearly to its natural density, may not separate totally from the hidrogen and nitrogen, in consequence of the reciprocal affinity of these three combustibles; as is shown by the habitual state of the coal in our fires, which always contains more or less of them: but the same cannot

but no oxigen.

be said of the oxigen. The condensation of the charcoal, carried to the degree that makes it appear black, is a state decidedly opposing the capacity of this principle to adhere to it as easily as the others. This admitted, if the charcoal in these bitumens be near its habitual density, we must not be surprised, that, remaining still united with the nitrogen and hidrogen, it cannot be equally so with the oxigen. On this principle neither is it strange to find, that pitcoal does not contain oxigen in a state to concur, during its distilla-

tion,

tion, in the formation of vinegar; as it does, when it is a constituent part of our vegetables.

But lastly, if nitric acid separate from these bitumens a charcoal united with nitrogen, such as distillation would furnish, it must be acknowledged, that coak exists free and condensed in their constitution; but not in that state of perfect combination, or mutual interlacement, which never fails to efface the characters of the elements of organized substances, as they reciprocally mask each other.

Coak exists in coal not chemically combined.

I have said enough I believe to show, that pitcoal, if it consist of vegetables, has retained nothing of the characters, that would approximate it rather to vegetable than animal substances. Vegetables, animals, bitumens, all have the same elements; that is nitrogen, hidrogen, oxigen, charcoal, sulphur, &c.: but the combination of these elements in pitcoal certainly does not in any respect resemble those, that vitality now forms in the beings arising before our eyes. Where in fact are the vegetables or animals, that contain charcoal simply deposited in their texture, as an oil or resin is in a plant? Besides, what organization could admit without inconvenience such a considerable excess of charcoal as that we see unemployed in these bitumens? Such a profusion certainly could only be an oversight of nature.

Coal retains nothing of the vegetable character.

We must therefore stop at one or other of the following consequences. Either nature was once capable of producing beings, the organization of which could admit so large a proportion of charcoal; and then the life, object, end, and means of existence of such beings, could in no respect be compared with those that now share the surface of the earth with us; and on this supposition pitcoal could only be the remains of animals or vegetables, which like so many others have disappeared from it for ever:

Either there must have been organized beings different from the present:

Or, if coal have originated from organic productions similar to ours, its interment has not only destroyed all marks of organization, but has displaced their elements, to reframe them, and fabricate with them those fossil masses, which have indeed retained all the combustibility of their nature, but in which we discern no trace of vegetation or animalization, no indication of the part they had to perform on the surface of the earth.

or their elements must have been separated, and combined in a different way, to form coal.

Oxide

Oxide of Charcoal.

Charcoal unites with oxigen to form an oxide.

Charcoal and oxigen are susceptible of a kind of union, which does not appear to me to have been noticed. It is totally different from carbonic acid, and from gaseous oxide of carbon. These always take place between *carbon* and oxigen; but the other readily admits charcoal, whether azotized or not; such indeed as we burn. The following are the facts.

Coal treated with nitric acid, gains an increase.

A hundred parts of coal of Villanueva, the coak of which amounts to 68, are increased by the application of nitric acid at 18° or 20° to 120 or 121 parts. Hence it follows, that, if this acid destroy by oxidation all the principles contained in this bitumen between 68 and 80, it leaves in their place first 32, and next 29, of some other principle, the nature of which we shall soon perceive.

The coal of Belmez likewise yields 120 or 121; that of Langreo the same; and the English, which leaves 64 of coak, produces 116, which amount to nearly the same.

This owing to a real combination.

That this is not owing to any thing merely imbibed, or to defective washing of the product, is decisively proved by the following experiment. Five drachms of Villanueva coal became 6 by the application of nitric acid. These 6 drachms were thrown into boiling water; but, after they had been collected and dried, they returned precisely to the weight of 5 drachms. Such a result clearly shows a surcharge, which, being of 50 or 52, amounts to half the coal employed. It is now time to make known its nature.

Oxide of charcoal, heated, in a retort,

Hold in the hand a small retort containing one or two hundred grains of oxided charcoal, its belly being at some distance above a chafingdish, so as to receive a gentle heat, and its beak being kept under water, to give vent to the atmospheric air, which the aqueous vapour begins to expel;

detonates obscurely.

and the instant the powder is agitated by a rapid movement of ebullition, move the beak under an inverted jar filled with water. This movement, which raises up the charcoal briskly, is a strong but obscure detonation, which terminates quickly, and without the least danger. At the same time a copious dew is produced, which, by its precipitation at its exit,

exit, always carries some powdered charcoal out of the retort.

The gasses in this experiment are such as might easily be foreseen: a mixture of carbonic acid and gaseous oxide of carbon, burning with a blue flame, and not detonating. If it be yellow, it is owing to the charcoal having retained some nitric acid. This is perceived too by its reddening with oxigen gas. Evolves carbonic acid and gaseous oxide.

From the great quantity of water formed during this distillation, I was led at first to believe, that the oxigen of the nitric acid, hidrogen, and charcoal, might form together a sort of union comparable to that of a vegetable oxide, which a higher temperature would occasion to be converted into water, carbonic acid, and gaseous oxide of carbon. And perhaps it is so. Yet the experiment I made immediately with powder of fir charcoal induced me, to consider the phenomena as belonging exclusively to charcoal and oxigen. Much water formed.

Let nitric acid of 20° or 25° be boiled on calcined charcoal of fir, elm, or other wood, reduced to powder; the charcoal, after being washed and dried, will commonly have a surcharge of twelve or thirteen per cent. If it be heated with the precautions already given, it will detonate with agitation, and afford the two gasses abovementioned, without any mixture of nitrous gas. Other experiments will be seen presently to prove, that this charcoal is in a state of peculiar combination, and not contaminated by remains of nitric acid. Wood charcoal treated with nitric acid.

I cannot fix with precision the method of thus oxiding vegetable charcoal, because I have found it variable. But it is not the same with that of pitcoal, the state of extreme division in which it is when deprived of hidrogen by the acid facilitating that union, so as not to leave us to grope our way. This more variable.

Liquid potash, even at a boiling heat, has no action on pitcoal; but if heated for a moment, in a very dilute state, on its oxide, or that of fir charcoal, a coffee coloured solution holding a considerable quantity is obtained, which is not altered by standing, or by the addition of water. Oxide of charcoal soluble by potash:

Ammonia acts with equal efficacy on them. A hundred grains of oxide of charcoal from Villaneuva coal dissolved in and by ammonia.

in it all except 15 grains, perhaps either because they were not oxided, or because the oxigen had accumulated in the rest, to facilitate its solution. From this solution an acid threw down a precipitate, which was black and shining when dry. It did not melt, and exhaled no smell on burning coals.

Oxigenized muriatic acid precipitated it, without acting on the precipitate.

The solution, precipitated, yielded silex, alumine, and iron.

This same charcoal, oxided, dissolved, and precipitated, left when burnt gray ashes, a little ferruginous, in which I found, silex, alumine, and a little oxide. What sort of union is there then between charcoals and their ashes? We should suppose potash could not dissolve the latter. I applied nitric acid to different sorts of charcoal, to divest them of ashes, and try whether they would burn away entirely; but I could not succeed.

What sort of combination is this?

I had intended to pursue the inquiry farther, but have been prevented. Meantime it will naturally be asked, what combination of oxigen and charcoal is this, that excludes neither nitrogen, nor any of the foreign substances it usually contains? and again what kind of union can charcoal form with the same principle, without losing any thing of its density or colour, which appears so little conformable to the laws of combination? I can only say, I know nothing on the subject.

We know less of charcoal than of carbon.

I would likewise ask, what is the nature of the combination that charcoal enters into with hidrogen and nitrogen, and other gasses which it absorbs and condenses in such large quantity? In fact we are far from being as well acquainted with charcoal, which we see daily, as with carbon, which never comes under the cognizance of our senses.

Dishidrogenized coal.

Coal divested of hidrogen.

When we consider how much the proportion of coak varies in pitcoals, we shall find it difficult not to believe, that, if these bitumens be really derived from plants like ours, the concentration of the carbonaceous principle, which is nothing but the loss of hidrogen, must have varied considerably in different countries. A coal that yields ·77 of coak cannot contain

contain as much hidrogen as another, that yields but ·57 or ·60.

*. This diminution of hidrogen has been carried so far in some coals, that from a total loss of hidrogen they are pure charcoal.

But if hidrogen have been capable of withdrawing entirely from certain coally masses; which will not appear surprising: when we recollect, that the carbonaceous principle in them is very near that degree of condensation which does not admit combination; it is not the same with nitrogen. This appears to be the last of the movable principles to quit it. Nitrogen adheres to it more strongly.

Near the monastery of Harbas, in the mountainous defile that separates the Asturias from the province of Leon, there is a vein of coal, perfectly resembling that of Quiros in its foliated texture and shining blackness. In this the carbonaceous principle is ·93, while the hidrogen is absolutely 0. In fact it is a true native coak, that burns without any bituminous or sulphurous smell, and leaves ·07 of white ashes. Native coak.

If, notwithstanding all its characters combined, it can be supposed, that this was never the base of a bitumen, I would urge the following experiment. Let it be heated with potash, and prussic lixivium will be obtained from it. The nitrogen, though the sole remain of the principles that have disappeared, here assists the chemist to trace its origin. It incontestably indicates, that it has belonged to that organic matter, which the hand of time has decomposed, to reduce to pitcoal. Still contains nitrogen.

Jet.

It is said that trunks of trees are found in collections, one extremity of which is converted into jet, while the other is still ligneous. If it be so, the analysis of such specimens would be exremely interesting. Trunks of trees half jet, half wood?

There is a great deal of difference in jets. A jet from Almagra in Murcia gave ·46 of charcoal without even softening: another, wrought into buttons, melted like a resin, and yielded ·52. Jets differ.

The following results to a certain point assimilate jet with our vegetables.

Heat

Its resemblance to vegetables.

Heat it with an acid at 20°, immediately a bulky effervescence takes place. As the solution advances, a deep yellow or annotta coloured concretion forms. The ebullition should be continued a little while, to collect all that can be formed. This substance, which is soft while the liquor is hot, is easily softened and separated. On washing it in boiling water, this acquires a yellow colour, but does not dissolve it. When dry it is bulky, bitter, and soluble in alcohol. But what is remarkable is its property of detonating obscurely with a very moderate heat like oxide of charcoal, either in a retort, or held over the flame of a candle on paper.

Products.

Its products are water, a little oil, ammonia, carbonic acid gas, gaseous oxide of carbon burning with a blue flame, and ·40 of a coal more bulky than the original substance.

The liquor that remains after its separation is of a deep yellow, and very bitter. It yields crystallized oxalic acid, and benzoic acid.

Compared with coal.

Pitcoal heated with an acid of 40° dissolves slowly, does not afford any charcoal, and yields the detonating substance, but with more difficulty, and in less quantity, than jet. Cannel coal comports itself like common coal, and not like jet. Some jet however does not yield the detonating substance without an acid of 40°, or with as much difficulty as pitcoal.

Hidrogen not necessary to the oxide of charcoal.

If we consider, that the first action of nitric acid on pitcoal consists in destroying the hidrogen, we shall easily conceive, that this hidrogen is not necessary to the formation of the detonating substance. The following experiment evidently shows this. Oxided Villanueva coal certainly contains no hidrogen: yet, treated with an acid of 40°, it affords the detonating substance; and since this yields water, ammonia, oil, &c., it is evident, that the nitrogen, the oxigen of the nitric acid, and the charcoal which the water is capable of furnishing by the concurrence of affinities that bring on its decomposition, establish themselves in a fixed proportion, to give rise to this singular product. A vegeto-animal compound, an artificial oxide of this nature, removes the difficulties I at first found in the formation of tannin from the simple presence of nitric acid and charcoal.

Illustrates the formation of tannin.

Acid

Acid of 35° or 40°, heated with fir or elm charcoal, at first gives out the gasses, that might be expected: but at a certain point the gasses cease to be formed, and the charcoal enters into solution. I have very old solutions of this kind, that are not altered. Alkalies precipitate nothing from them, because charcoal, either oxided or in a state of simple division, is as soluble in alkalis as in acids. I had proceeded thus far, when I learned from the Chemical Annals, that tannin was formed by the solution of carbonaceous substances. I believe Crell is the first, who announced the solution of charcoal in nitric acid.

Nitric acid dissolves charcoal.

A hundred grains of Villanueva coal, treated with concentrated sulphuric acid, and perfectly washed and dried, left 104. Was this coal oxided? I find nothing mentioned in my notes, but that it burned without any bituminous smell, that of sulphur only being noticeable.

Coal treated with sulphuric acid.

Turf.

A Piece of turf from Dax afforded me the following results.

Turf

A hundred parts left 40 of charcoal without any change of bulk. Its products were water, and vinegar mingled with ammonia, the taste of which did not differ from that of wood: but a yellow oily vapour came over, that became hard like suet, which does not commonly occur in the distillation of wood. This suet was from 6 to 6½ per cent. I did not examine the gasses.

yields ·40 of charcoal, water, vinegar, ammonia, and ·6 of a sebaceous oil.

Potash, which has not the least action on pitcoal, to my astonishment perfectly dissolved the turf. The solution is coffee-coloured. Acids decompose it, throwing down a brown flocculent precipitate, which distillation renders black, shining, and fragile. When distilled it is converted into charcoal, without softening or diminishing in bulk; yields the butyraceous product; and is reduced to ·50 of charcoal.

Potash dissolves turf.

Nitric acid at 30° does not decompose turf, as it does pitcoal. Washed and dried it affords water, and the butyraceous matter, but no acid. It might be supposed, that this suet exists ready formed in turf. This deserves to be farther

Nitric acid does not decompose it.

ther examined, and particularly to be compared with those plants, the bituminization of which is not far advanced, to see what changes its progress effects in their constitution.

By boiling dissolves it.

Nitric acid of 40°, with a boiling heat, dissolves turf. Crystallizable oxalic acid is obtained from it, and the yellow bitter matter; but no detonating product, like that of jet. I know not whether Hatchett's tannim be found in it.

Incinerated.

Its incineration is tolerably slow. Its coal does not emit the ammoniacal smell of azotized charcoals. Its ashes are gray, without the least indication of saline matter or lime. Their lixivium does not alter the juice of the blue-bottle. Acids do not occasion the slightest effervescence with them. They contain a great deal of silex, sulphate of lime, and a little magnesia.

II.

Abstract of a Memoir on Muriatic Ether, read at the Institute February the 17th, 1807, by Mr. THENARD*.

Muriatic ether hitherto unknown.

AFTER having examined why muriatic ether has remained hitherto unknown to chemists, though it has been repeatedly an object of their research, the author gives the process for obtaining it. As it is habitually in the state of gas, the following apparatus must be employed.

A gas at the common temperature.

Arrangement of the apparatus for obtaining it.

Into a retort, capable of containing no more than the mixture in its belly, equal parts by measure of highly concentrated muriatic acid and alcohol at 36° are to be put, and well shaken, to bring all the particles of each into contact. This done 7 or 8 grains of sand at most are to be thrown into the retort, to prevent the sudden ebullitions that might otherwise take place in the course of the process: after which it is to be supported on the naked fire of a common furnace by a grate of iron wire, and a Welter's tube adapted to it, terminating in a threenecked bottle, the capacity of which is double that of the retort, and which must be half

* Annales de Chimie, vol LXI, p. 291, March, 1807: and Journal de Physique, vol. LXIV, p. 260.

filled

filled with water at 20° or 25° [66° or 72° F.]. The tube must be immersed in the water to the depth of 7 or 8 cent. [2¾ or 3 in.]; a straight tube of safety must be introduced into the central tubulure; and from the third a curved tube must proceed, opening under inverted phials filled with water at the same temperature in an earthen bowl.

Process.

The apparatus being thus arranged, the retort is to be heated gradually; and twenty or five and twenty minutes after the fire is kindled bubbles will be seen to rise from the lower part of the fluid, particularly from the surface of the grains of sand. These bubbles presently become more numerous, and abundance of ethereous gas is soon obtained. Acid, alcohol, and water, at the same time pass over, but remain in the first phial. From 500 gr. [7722½ grs.] of acid, and an equal bulk of alcohol, upward of 20 litres [wine quarts], or even as far as 30, of ethereous gas, perfectly pure, may be obtained. Much more will be obtained, if, as soon as the extrication of gas begins to slacken, fresh alcohol be added to the residuum; that is, to the strongly acid residuum, which remains in the retort, and will then be about two fifths of the bulk of the original mixture. I even think, that, if hot alcohol were occasionally poured into the retort through a tube 6 or 7 decim. [24 or 27 in.] long reaching to its bottom, the formation of etherized gas would be still more abundant; for it is obvious, that more alcohol than muriatic acid rises every instant, thus therefore we should reestablish their original proportions, which are best adapted to the success of the process. In all cases the management of the fire is of the highest importance: if it be too weak, it will produce no etherized gas; if it be too strong, it will produce but little. Neither will the alcohol be etherified in any sensible degree by loading it with muriatic acid gas, or by causing the alcohol and acid both to meet in vapour in a tube about 80° [174½° F.]. It is only therefore by maintaining a due medium in the application of the fire, that we can succeed completely. The cause of this is, that too great or too little elasticity in the alcohol and in the muriatic acid is injurious to their mutual action on each other. Another precaution to be taken is, to use the same water for collecting

Proportion of gas obtained.

Probably still more by particular management.

Great attention to the fire requisite.

If the acid, or both, be in the state of gas, little or no ether will be formed.

The water dissolves a certain quantity.

lecting the gas, and to employ as little as possible, because it dissolves a certain quantity of it.

Characters of the gas.

The gas is perfectly colourless; it has a strong smell of ether; and its taste is perceptibly saccharine. It has no action whatever on infusion of litmus, sirup of violets, or limewater. Its specific gravity, compared with that of the air, is 2·219 at 18° [63° F.], and 75 cent. [29·4 in.] of atmospheric pressure. At this pressure water dissolves its own bulk. At the same pressure, but at the temperature of 11° [50° F.], the gas assumes the liquid state.

Method of obtaining the ether in a liquid state.

A large quantity may be procured in this state by employing an apparatus similar to that already described; only instead of causing the last tube to open under a bottle filled with water it must be made to reach to the bottom of a long, narrow vessel, well dried, and surrounded with ice, which must be renewed as it melts. The ethereous gas will arrive alone in this, and be completely condensed: for as soon as all the common air is expelled from the vessels, their communication with the atmosphere may be cut off without danger.

Characters of the liquid ether.

This ether in the liquid state is remarkably limpid. Like the gas it is colourless, and has no action on litmus or sirup of violets: is very soluble in alcohol, from which it may in great part be separated by water: has a very decided smell, and a very distinct taste resembling that of sugar, which is particularly observable in water saturated with it. Probably it may thus be employed with success as a medicine. Poured on the hand it suddenly enters into ebullition, and produces considerable cold, leaving a small whitish sediment. At the temperature of 5° [39·2° F.] its weight is to that of water as 874 to 1000. Thus, though it is much more volatile than sulphuric ether, and of course than alcohol, it is not only heavier than the first, but even than the second of these. Finally it does not congeal at a temperature of $-29°$ [22·2° below 0 F.].

Thus far we see nothing in this ether but agrees with the phenomena exhibited by other bodies. It is an object of curiosity only on account of its novelty, and the facility with which it is converted into a gas or a liquid. But if we study it farther, it will appear one of the most singular and extraordinary compounds we can form. It does not redden

A singular compound.

the

the most dilute infusion of litmus; the strongest alkalis have no action on it; the solution of silver is not rendered in the least turbid by it: and all this whether it be used in the gaseous or liquid state, or dissolved in water. But set it on fire, and immediately such a large quantity of muriatic acid is developed, that it precipitates a concentrated solution of nitrate of silver in a solid mass, suffocates those who inhale it, and is even visible in the form of vapour in the surrounding air.

No test indicates the presence of muriatic acid in it, yet it gives out a great quantity when burned.

Is the muriatic acid formed in this combustion, as we might be tempted to suppose? or is it only set at liberty, which is possible? These questions Mr. Thenard afterward endeavours to solve.

Is the acid then formed, or only set free?

If the muriatic acid be formed in the combustion of the ethereous gas, the radical of this acid must exist in the gas; and it must necessarily proceed from the alcohol, or from the muriatic acid decomposed by the alcohol, or, which is improbable, though not impossible, from both together. In the first case, on distilling a mixture of muriatic acid and alcohol, we should find after the distillation all the muriatic acid employed, beside what arises from the combustion of the gas formed. In the second case, on the contrary, a large quantity of acid ought to disappear in the distillation: but the whole of this quantity, and no more, ought to reappear on the combustion of the acid formed. In the third case, a loss of acid should be occasioned by the distillation; but this loss should be more than compensated by the quantity of acid, which is produced from the combustion of the gas formed.

If the former, its radical must come from the alcohol, the acid, or both.

Tests of these three suppositions.

Now if the process be performed with 450·937 gram. [6962·722 grs.] of muriatic acid, of the specific gravity of 11·349, at temperature 5° [39·2° F.], with an equal bulk of highly rectified alcohol, 23 lit. [quarts] of ethereous gas will be formed at the temperature of 21° [68° F.] and pressure ·745 met. [29·2 in.], and 122·288 gram. [1888·738 grs.] of acid disappear. Consequently the first supposition is false, since it is demonstrated, that, if the radical of the muriatic acid exist in the ethereous gas, it must proceed, not from the alcohol solely, but either from the muriatic acid alone, or from the muriatic acid and alcohol.

Proof that the radical does not come from the alcohol alone.

It might proceed from the acid alone in two ways.

Let us see whether it can proceed from the muriatic acid alone, agreeably to the second supposition. There are two ways in which this may be conceived to take place: either the muriatic acid may have been decomposed by the alcohol, so that its radical is found in the ethereous gas separated from its other principle: or the decomposition may have taken place in such a way, that both the principles of the muriatic acid exist in the ethereous gas, not united together, not forming muriatic acid, but combined with the principles of the alcohol, in the same state in which hidrogen, oxigen, carbon, and azote, are found in vegetable and animal substances.

If the radical alone enter in to the gas, it cannot reproduce the acid when burned without access of air.

If all its principles enter into it, the whole may thus be reproduced

Now if the radical of the muriatic acid exist alone, or without some part of the other principles, in the ethereous gas, we ought to obtain no acid, when we decompose this gas in a redhot tube with exclusion of air, or less than disappeared in the experiment that produced it. But if the gas contain not only the radical of the muriatic acid, but all its constituent principles; as the principles of this acid, whatever they are, have a great tendency to combine together, we may presume, that, on destroying the ethereous gas by fire without the contact of air, we shall probably obtain the whole quantity of muriatic acid, that disappeared in the experiment in which the gas was formed.

The gas decomposed in close vessels.

It was of the highest importance therefore, to effect this decomposition in close vessels. This was accordingly done with 900 gram. [29 oz. troy] of concentrated muriatic acid, and an equal bulk of well rectified alcohol. Between the redhot tube of glass, in which the gas was decomposed, and the retort where it was produced, a large tubulated bottle was placed containing water at 15° or 16° [57° or 59° F.], to retain the acid, alcohol, and water, that might rise with the gas. The glass tube had a communication also with two other bottles, one containing water, the other potash, to absorb all the acid that might reappear in the operation. Lastly by means of another tube the gasses were collected. That this operation may be attended with success, the glass tube must be well coated, and the fire cautiously managed, to prevent it from melting. Though

near

near 50 litres of ethereous gas must have been produced in this experiment, and near 250 gram. of acid have disappeared in the first instance, yet the whole of the acid, except 4 gram. [62 grs.] reappeared in the redhot tube, and were dissolved in the last two bottles of the apparatus.

Nearly the whole of the acid reproduced.

Thus of all the suppositions above made, which are the only ones that can reasonably be formed considering the muriatic acid as a compound, there is only one admissible; which is, that the elements of the muriatic acid exist in the ethereous gas combined with those of the alcohol, in the same manner as the elements of water, carbonic acid, ammonia, &c., exist in vegetable and animal substances.

The elements of the acid therefore exist in the gas, if it be a compound.

But if we suppose the muriatic acid to be a simple substance, we must necessarily consider the ethereous gas as formed of muriatic acid and alcohol, or as a substance derived from the decomposition of the alcohol: for perhaps the alcohol is decomposed when we distill it with muriatic acid, at least this will easily be seen by and by. The question therefore being reduced to a choice between these two hypotheses, let us endeavour as far as possible to discuss their weight.

Supposition that the acid is a simple substance.

The latter presents us with phenomena very difficult to explain. In fact we must suppose that the alcohol, or the principle it contains, acts on the muriatic acid with much more energy than the strongest alkali; since this alkali cannot take the acid from it, and muriate of potash, as I shall hereafter show, contains less acid than the ethereous gas. How too can we conceive, that nitrate of silver, which takes the whole of the muriatic acid from muriate of potash, cannot take any from the ethereous gas, which contains still more?

Difficulties that may be objected to it.

On the other supposition, on the contrary, every thing is naturally explained. We see why the ethereous gas does not redden the infusion of litmus; why alkalis do not affect it; why nitrate of silver does not produce a precipitate with it; and why on burning it so large a quantity of muriatic acid is generated, that it appears in the surrounding air in the form of vapour: in short every thing is reconcilable with the phenomena exhibited by other substances.

The other naturally accounts for every thing.

Mr. Thenard however is far from absolutely adopting one hypothesis and rejecting the other. Both deserve to be investigated

Yet it cannot be considered as absolutely decided.

tigated, and on this he is at present eagerly engaged, since, however it may turn out, the results cannot fail to be very important.

Note on the Discovery of the Muriatic Ether; by Mr. THENARD.

The muriatic ether was unknown in France,

When on the 18th of February last I read to the Institute my paper on muriatic ether, all the members, among whom were Messrs. Berthollet, Chaptal, Deyeux, Fourcroy, Guyton, Vauquelin, and Gay-Lussac, considered the results it contained as perfectly novel, and were struck with the conse-

and in Spain;

quences, that might be deduced from them. Mr. Proust, who is at present in Paris, and before whom I was eager to repeat, at his desire, the experiments I had made with the ethereous gas, was not less surprised than the French chemists. But last friday, that is twenty-five days after I had read my paper,

but not in Germany.

Mr. Gay-Lussac, turning over Gehlen's Journal, accidentally found in a note, that Gehlen himself had made experiments on muriatic ether, and recorded them in one of the volumes of his Journal published in 1804.

Gehlen made it in different ways in 1804.

It appears, that Mr. Gehlen made muriatic ether from equal weights of the fuming muriate of tin and alcohol. He likewise made it in Basse's method, by a mixture of seasalt, concentrated sulphuric acid, and alcohol; from which till Basse's time, and even Gehlen's, sulphuric ether only was supposed to be obtained. He did not obtain any with muriatic acid alone. Mr. Gehlen however observed most of the properties in muriatic ether, that I have mentioned. The chief differences between us are, that he has not attempted to investigate the source of the muriatic acid produced by burning the gas, the quantity it is capable of affording, or the theory of the formation of the ether. My process too not

His was less in quantity, and less pure.

only affords ether in larger quantity probably than any other, but much purer; for the specific gravity of mine was ·874, of his only ·845, and in this case the greater gravity is a proof of greater purity. He likewise marks the point of its condensation as about 10° of Reaumur [54·5° F].

As from the account of Mr. Gehlen I could no longer doubt but muriatic ether had been made in Germany, and its property

property of yielding a large quantity of muriatic acid in burning observed; and at the same time convinced, that a fact so important was altogether unknown both in France and Spain; I was desirous of satisfying myself, whether the English chemists were any farther advanced in this respect. For this purpose I addressed myself to Mr. Riffault, who is translating the third edition of Thomson's Chemistry, a work of great erudition, begun long after Gehlen's paper was published. Mr. Riffault read to me every thing in it concerning muriatic ether. No mention is made there of Gehlen, or of the singular properties of the muriatic ether. Hence I consider myself authorized to conclude, that the muriatic ether was unknown in England, as well as in France and Spain; and that, without having information of Gehlen's labours, I may claim at least the merit of having made it known here. How often has it already happened, that a discovery has been made in one country some years after it had been made in another; and this because unfortunately all learned men do not speak the same language, and all the works published in one are far from being translated into the rest.

Not known in England.

Discoveries often made a second time, the first remaining unknown to foreigners.

In an additional note Mr. Thenard says, that, ten days after his paper was read, Mr. Boullay, an apothecary of Paris, acquainted him, that he had likewise formed muriatic ether directly from muriatic acid and alcohol, but had not made it public, because he had not yet completed his labours on the subject.

Mr. Boullay too had made it.

III.

Abstract of a Memoir on the Products that result from the Action of Metallic Muriates, oxigenized Muriatic Acid, and Acetic Acid, on Alcohol: by Mr. THENARD*.

IN this paper Mr. Thenard shows, that the metallic muriates form but a very small quantity of ether with alcohol: that this ether, which at first is found dissolved in a large quantity of alcohol, may be separated from it in the state of gas

Metallic muriates form but little ether with alcohol.

* Annales de Chimie, vol. LXI. p. 308, March, 1807.

gas by a gentle heat; particularly by means of hot water, which seizes on the alcohol, and to a certain point sets the ether at liberty: that this ethereous gas has the greatest analogy with that which is obtained from muriatic acid and alcohol: that both have the same smell, taste, and solubility in water, and burn in the same manner with a green flame, and diffusing vapours of muriatic acid, though previous to the combustion no test can detect the presence of any in the gas: and that they differ only in this, that the muriatic ethereous gas is not condensible at a temperature above 12·5° [52·7° F.], while the other is at 16·5° [60° F.]

Not so volatile as that from the acid.

Formed wholly from the excess of acid in the muriate.

This difference being but slight, Mr. Thenard conceives, that the nature and mode of formation of both are the same; and that in the metallic muriates it is only the excess of acid that acts on the alcohol. For this reason a large quantity of metallic muriate is necessary to convert alcohol into ether; and it is the more easily effected, in proportion as the muriate contains a greater excess of acid, and is more soluble in alcohol. Hence the process succeeds better with the muriate of tin than with any other. In every case the muriate is not disoxigenized, and a portion of the oxide only is found to be precipitated.

Oxigenized muriatic acid decomposed by alcohol.

Considering afterward the action of oxigenized muriatic acid on alcohol, he shows, that in the mutual action of these two substances on each other, which is very powerful, almost all the oxigenized muriatic acid is decomposed: and the result is a great deal of water; a great deal of muriatic acid; some alcohol not decomposed; a tolerable quantity of an oily matter heavier than water, having a cool taste analogous to that of mint, a peculiar smell differing from that of ether; and beside these a small quantity of carbonic acid, of a substance easily carbonized, and probably of acetous acid, but no ether. Farther, that the oxigenized muriatic ether of Scheele is nothing but muriatic ether properly so called, when made with a mixture of alcohol, muriatic acid, and black oxide of manganese; or a mixture of muriatic and sulphuric ether, when made from black oxide of manganese, common salt, alcohol, and sulphuric acid: that Pelletier's is of the same nature, since he made it of a similar mixture: and that what

But no ether produced.

Mistake of Scheele in this respect.

what is said to be obtained by passing oxigenized muriatic acid gas through alcohol is nothing but a solution of more or less of the oily matter in alcohol. The oil may even be separated from the alcohol by the addition of water, and the same compound formed again by dissolving this oil in a given quantity of alcohol.

Solution of a peculiar oil is alcohol mistaken for ether.

What is new in this part of Mr. Thenard's labours is not the formation of the oily matter, water, acetous acid, &c., by the decomposition of oxigenized muriatic acid: for Scheele, in his Chemical Essays, speaks of the oily matter; and Berthollet, in the Memoirs of the Academy for 1785, speaks not only of this, but of the water, acetous acid, &c., formed in the process: but it is the having proved*, that oxigenized muriatic acid cannot form ether with alcohol, and having explained why Scheele and so many other chemists did obtain some.

Finally, desirous of examining the formation of acetic ether, Mr. Thenard mixed together 120 gram. [1853 grs.] of alcohol as highly rectified as possible, and as much acetic acid, of an acidity determined by the quantity of potash the acid requires to saturate it. He distilled the mixture, cohobated it twelve times, and thus evidently decomposed all the alcohol employed, beside 66·16 gram. [1022 grs.] of acetic acid, answering to 32 gram. [509 grs.] of dry acid, or such as it exists in acetate of potash well fused. Yet about 120 gr. [1853 grs.] only of acetic ether were formed; though no gas was evolved, and, when the process was finished, a loss of 7 gram. [108 grs.] only appeared. Hence Mr. Thenard is led to suppose, that part of the oxigen of the acetic acid combines with part of the hidrogen of the alcohol, while the other principles of the acid, and those of the alcohol, unite to form the ether. Otherwise, if no water were formed, we must admit, in order to account for the appearances, that the best rectified alcohol contains nearly a fifth of its weight of water, which is scarcely probable.

Acetic ether.

Water formed in this process

This

* Mr. Berthollet even mentioned, that the oxigenized muriatic acid and alcohol produced but very little ether; and we perceive, that he inclined to consider this small quantity of ether as foreign to the mutual action of these two substances.

Characters of acetic ether.

This ether has an agreeable smell of ether and acetic acid; yet it reddens neither the infusion nor paper of litmus: and it has a peculiar taste, not very different from that of alcohol. Neither its specific gravity nor degree of elasticity was accurately ascertained: but it is lighter than water, and swims on it, and heavier than alcohol. Water appears to dissolve much more than it does of sulphuric ether. It burns with a yellowish white flame, and produces an acid, which is probably the acetic. It does not appear to undergo any alteration by keeping; at least it did not in the course of six months.

IV.

Some Hints respecting the proper Mode of inuring Tender Plants to our Climate. By the Right Hon. Sir JOSEPH BANKS, *Bart. K. B. P. R. S. &c*.*

Interesting and advantageous to harden tender plants.

RESPECTABLE and useful as every branch of the horticultural art certainly is, no one is more interesting to the public, or more likely to prove advantageous to those who may be so fortunate as to succeed in it, than that of inuring plants, natives of warmer climates, to bear without covering the ungenial springs, the chilly summers, and the rigorous winters, by which, especially for some years past, we have been perpetually visited.

Attempts in this way.

Many attempts have been made in this line, and several valuable shrubs, that used to be kept in our stoves, are now to be seen in the open garden; there is, however, some reason to believe, that every one of these was orginally the native of a cold climate, though introduced to us through the medium of a warm one; as the gold tree, aucuba japonica, the moutan, pæonia frutescens, and several others have been in our times.

Annuals.

In the case of annuals, however, it is probable that much has been done by our ancestors, and something by the present generation; but it must be remembered, that all that is required in the case of an annual is, to enable it to ripen

* From the Transactions of the Horticultural Society, Part I, p, 21.

its

its fruit in a comparatively cold summer, after which we know that the hardest frost has no power to injure the seed, though exposed in the open air to its severest influence; but a perennial has to encounter frosts with its buds and annual shoots, that have sometimes been so severe with us, as to rend asunder the trunks of our indigenous forest trees.

Wheat. It is probable that wheat, our principal food at present, did not bring its seed to perfection in this climate, till hardened to it by repeated sowings; a few years ago some spring wheat from Guzerat was sown with barley, in a well cultivated field: it rose, eared, and blossomed, with a healthy appearance, but many ears were when ripe wholly without corn, and few brought more than three or four grains to perfection.

Water oat. In the year 1791, some seeds of zizania aquatica were procured from Canada, and sown in a pond at Spring Grove, near Hounslow; it grew, and produced strong plants, which ripened their seeds; those seeds vegetated in the succeeding spring, but the plants they produced were weak, slender, not half so tall as those of the first generation, and grew in the shallowest water only; the seeds of these plants produced others the next year sensibly stronger than their parents of the second year.

Gradually acclimated. In this manner the plants proceeded, springing up every year from the seeds of the preceding one, every one becoming visibly stronger and larger, and rising from deeper parts of the pond, till the last year, 1804, when several of the plants were six feet in height, and the whole pond was in every part covered with them as thick as wheat grows on a well managed field.

till perfectly vigorous in 14 years. Here we have an experiment which proves, that an annual plant, scarce able to endure the ungenial summer of England, has become, in fourteen generations, as strong and as vigorous as our indigenous plants are, and as perfect in all its parts as in its native climate.

Bay tree. Some of our most common flowering shrubs have been long introduced into the gardens; the bay tree has been cultivated more than two centuries; it is mentioned by Tusser, in the

list

list of garden plants inserted in his book, called 500 points of good Husbandry, printed in 1573.

Laurel.

The laurel was introduced by Master Cole, a merchant living at Hampstead, some years before 1629, when Parkinson published his Paradisus Terrestris, and at that time we had in our gardens, oranges, myrtles of three sorts, laurustinus, cypress, phillyrea, alaternus, arbutus; a cactus brought from Bermudas, and the passion flower, which last had flowered here, and showed a remarkable particularity, by rising from the ground near a month sooner if a seedling plant, than if it grew from roots brought from Virginia.

Orange, myrtle, &c.

Passion flower remarkable particularity.

All still tender.

All these were at that time rather tender plants; Master Cole cast a blanket over the top of his laurel, in frosty weather, to protect it, but though nearly two centuries have since elapsed not one of them will yet bear with certainty our winter frosts.

Have not been propagated by English seed.

Though some of these shrubs ripen their seeds in this climate, it never has been, I believe, the custom of gardeners to sow them; some are propagated by suckers and cuttings, and others by imported seeds; consequently the very identical laurel introduced by Master Cole, and some others of the plants enumerated by Parkinson, are now actually growing in our gardens; no wonder then, that these original shrubs have not become hardier, though probably they would have done so, had they passed through several generations by being raised from British seeds.

Plants propagated by cuttings or suckers do not grow hardy. Seed therefore should be tried.

Is it not then worthy a trial, as we find that plants raised from suckers or cuttings do not grow hardier by time, and as the experiment on zizania points out the road, to sow the seeds of these and such tender shrubs as occasionally ripen them in this climate. Fourteen generations, in the case of the zizania, produced a complete habit of succeeding in this climate, but a considerable improvement in hardiness was evident much earlier.

In plants that require some years to arrive at puberty, fourteen generations is more than any man can hope to survive; but a much less number will in many cases be sufficient, and in all, though a complete habit of hardihood is not attained, a great progress may be made towards it in a much

less

less time; even one generation may work a change of no small importance, if we could make the myrtle bear the climate of Middlesex, as well as it does that of Devonshire, or exempt our laurel hedges from the danger of being cut down by severe frosts, it would be an acquisition of no small consequence to the pleasure of the gentleman, as well as to the profit of the gardener.

Myrtle and laurel begun to be tried.

Old as I am, I certainly intend this year to commence experiments on the myrtle and the laurel: I trust, therefore, it will not be thought presumptuous in me to invite those of my brethren of this most useful Society, who are younger than I am, and who of course will see the effect of more generations than I shall do, to take measures for bringing to the test of experiment the theory I have ventured to bring forward, I hope not without some prospect of success.

New Holland promises many plants.

The settlement lately made at New Holland gives a large scope to these experiments; many plants have been brought thence which endure our climate with very little protection, and some of these arrive at puberty at an early period; we have already three from the south point of Van Diemen's Island, where the climate cannot be wholly without frost; mimosa verticillata, eucalyptus hirsuta and obliqua. The first of these appears to have produced flowers within eight years of its first introduction, but as a settlement is now made very near the spot where the seeds of these shrubs were collected, we may reasonably hope to receive farther supplies, and, among them, the winterana aromatica, an inhabitant of the inhospitable shore of Terra del Fuego, which Mr. Brown has discovered on the south part of Van Diemen's Island also.

V.

Observations on the Method of producing new and early Fruits. *By* THOMAS ANDREW KNIGHT, *Esq: F. R. S. &c.**

Gifts of nature to be improved by man.

NATURE has given to man the means of acquiring those things which constitute the comforts and luxuries of civilized

* Transactions of the Horticultural Society, Part I, p. 30.

life

life, though not the things themselves; it has placed the raw material within his reach; but has left the preparation and improvement of it to his own skill and industry. Every plant and animal, adapted to his service, is made susceptible of endless changes, and, as far as relates to his use, of almost endless improvement. Variation is the constant attendant on cultivation, both in the animal and vegetable world; and in each the offspring are constantly seen, in a greater or less degree, to inherit the character of the parents from which they spring.

Fruits best in peculiar soils, situation, and management.

No experienced gardener can be ignorant, that every species of fruit acquires its greatest state of perfection in some peculiar soils and situations, and under some similar mode of culture: the selection of a proper soil and situation must therefore be the first object of the improver's pursuit; and nothing should be neglected which can add to the size, or improve the flavour of the fruit from which it is intended to propagate. Due attention to these points will in almost all cases be found to comprehend all that is necessary to insure the introduction of new varieties of fruit, of equal merit with those from which they spring; but the improver, who has to adapt his productions to the cold and unsteady climate of Britain, has still many difficulties to contend with; he has to combine hardiness, energy of character, and early maturity, with the improvements of high cultivation. Nature has, however, in some measure, pointed out the path he is to pursue; and, if it be followed with patience and industry, no obstacles will be found, which may not be either removed, or passed over.

Hence new varieties.

Hardiness and early ripening desirable.

Plants carried to a hot or cold climate and brought back.

If two plants of the vine, or other tree, of similar habits, or even if obtained from cuttings of the same tree, were placed to vegetate, during several successive seasons, in very different climates; if the one were planted on the banks of the Rhine, and the other on those of the Nile, each would adapt its habits to the climate in which it was placed; and if both were subsequently brought, in early spring, into a climate similar to that of Italy, the plant which had adapted its habits to a cold climate would instantly vegetate, whilst the other would remain perfectly torpid. Precisely the same thing occurs in the hothouses of this country, where a plant accustomed

Hothouses.

accustomed to the temperature of the open air will vegetate strongly in December, whilst another plant of the same species, and sprung from a cutting of the same original stock, but habituated to the temperature of a stove, remains apparently lifeless. It appears, therefore, that the powers of vegetable life, in plants habituated to cold climates, are more easily brought into action than in those of hot climates; or, in other words, that the plants of cold climates are most excitable: and as every quality in plants becomes hereditary, when the causes which first gave existence to those qualities continue to operate; it follows that their seedling offspring have a constant tendency to adapt their habits to any climate in which art or accident places them.

Plants from cold climates earliest.

But the influence of climate on the habits of plants, will depend less on the aggregate quantity of heat in each climate, than on the distribution of it in the different seasons of the year. The aggregate temperature of England, and of those parts of the Russian empire, that are under the same parallels of latitude, probably does not differ very considerably; but, in the latter, the summers are extremely hot, and the winters intensely cold; and the changes of temperature between the different seasons are sudden and violent. In the spring, great degrees of heat suddenly operate on plants which have been long exposed to intense cold, and in which excitability has accumulated during a long period of almost total inaction; and the progress of vegetation is in consequence extremely rapid. In the climate of England, the spring, on the contrary, advances with slow and irregular steps, and only very moderate and slowly-increasing degrees of heat act on plants in which the powers of life have scarcely in any period of the preceding winter been totally inactive. The crab is a native of both countries, and has adapted alike its habits to both; the Siberian variety introduced into the climate of England retains its habits, expands its leaves, and blossoms on the first approach of spring, and vegetates strongly in the same temperature in which the native crab scarcely shows signs of life; and its fruit acquires a degree of maturity, even in the early part of an unfavourable season, which our native crab is rarely or never seen to attain.

Not the aggregate quantity of heat, but its distribution through the season, the chief point. England and Russia.

Siberian crab.

Similar

Annuals.

Similar causes are productive of similar effects on the habits of cultivated annual plants; but these appear most readily to acquire habits of maturity in warm climates; for it is in the power of the cultivator to commit his seeds to the earth at any season; and the progress of the plants towards maturity will be most rapid, where the climate and soil are most warm. Thus the barley grown on sandy soils, in the warmest parts of England, is always found by the Scotch farmer, when introduced into his country, to ripen on his cold hills earlier than his crops of the same kind do, when he uses the seeds of plants, which have passed through several successive generations in his colder climate; and in my own experience, I have found that the crops of wheat on some very high and cold ground, which I cultivate, ripen much earlier when I obtain my seed-corn from a very warm district and gravelly soil, which lies a few miles distant, than when I employ the seeds of the vicinity.

Seeds from a warm climate ripen earliest.

Esculent plants.

The value, to the gardener, of an early crop, has attracted his attention to the propagation and culture of the earliest varieties of many species of our esculent plants; but in the improvement of these he is more often indebted to accident than to any plan of systematic culture; and contents himself with merely selecting and propagating from the plant of the earliest habits, which accident throws in his way; without inquiring from what causes those habits have arisen: and few efforts have been made to bring into existence better varieties of those fruits which are not generally propagated from seeds, and which, when so propagated, of necessity exercise, during many years, the patience of the cultivator, before he can hope to see the fruits of his labour.

Attempts to produce early varieties of fruit.

The attempts which I have made to produce early varieties of fruit are, I believe, all that have yet been made; and though the result of them is by no means sufficiently decisive to prove the truth of the hypothesis I am endeavouring to establish, or the eligibility of the practice I have adopted, it is amply sufficient to encourage future experiment.

Apples.

The first species of fruit, which was subjected to experiment by me, was the apple; some young trees of those varieties

rieties of this fruit, from which I wished to propagate, were trained to a south wall, till they produced buds which contained blossoms. Their branches were then, in the succeeding winter, detached from the wall, and removed to as great a distance from it as the pliability of their stems would permit; and in this situation they remained till their blossoms were so far advanced in the succeeding spring, as to be in some danger of injury from frost. The branches were then trained to the wall, where every blossom I suffered to remain, soon expanded, and produced fruit. This attained in a few months the most perfect state of maturity; and the seeds afforded plants, which have ripened the fruit very considerably earlier than other trees, which I raised at the same time, from seeds of the same fruit, which had grown in the orchard. In this experiment the fecundation of the blossoms, of each variety, was produced by the farina of another kind; from which process, I think, I obtained, in this, and many similar experiments, an increased vigour and luxuriance of growth; but I have no reasons whatever to think that plants thus generated ripen their fruit earlier than others which are obtained by the common methods of culture. I must therefore attribute the early maturity of those I have described to the other peculiar circumstances under which the fruit and seeds ripened, from which they sprang.

Fruit fecundated by farina from another tree.

I obtained, by the same mode of culture, many new varieties, which are the offspring of the Siberian crab and the richest of our apples, with the intention of affording fruits for the press, which might ripen well in cold and exposed situations. The plants, thus produced, seem perfectly well calculated, in every respect, to answer the object of the experiment, and possess an extraordinary hardness and luxuriance of growth. The annual shoots of some of them, from newly grafted trees in my nursery, the soil of which is by no means rich, exceeded six feet and a half in height, in the last season; and their blossoms seem capable of bearing extremely unfavourable weather without injury. In all the preceding experiments some of the new varieties inherited the character of the male, and others of the female, parent in the greatest degree; and of some varieties of fruit (particularly the golden pippin) I obtained a better copy, by introducing

Siberian crab and a rich apple combined to produce cider fruit.

Variety of the golden pippin.

troducing the farina into the blossom of another apple, than by sowing their own seeds; I sent a new variety (the Downton pippin) which was thus obtained from the farina of the golden pippin, to the Horticultural Society, last year, but those specimens afforded but a very unfavourable sample of it; for the season, and the situation in which the fruit ripened, were very cold, and almost every leaf of the trees had been eaten off by insects. In a favourable season and situation it will, I believe, be found little, if at all, inferior to the golden pippin, when first taken from the tree; but it is a good deal earlier, and probably cannot be preserved so long.

Grape.

I proceed to experiments on the grape; which, though less successful than those on the apple, in the production of good varieties, are not less favourable to the preceding conclusions.

Vinery without fires.

A vinery in which no fires are made during the winter, affords to the vine a climate similar to that which the southern parts of Siberia afford to the apple, or crab tree: in it a similarly extensive variation of temperature takes place, and the sudden transition from great comparative cold to excessive heat is productive of the same rapid progress in the growth of the plants, and advancement of the fruit to maturity.

Black cluster united with the sweetwater,

My first attempt was to combine the hardiness of the blossom of the black cluster, or Burgundy grape, with the large berry and early maturity of the true sweetwater*. The seedling plants produced fruit in my vinery at three or four months old, and the fruit of some of them was very early; but the bunches were short, and ill formed, and the berries much smaller than those of the sweetwater, and the blossoms did not set by any means so well as I had hoped.

and with the chasselas.

Substituting the white chasselas for the sweetwater, I obtained several varieties, whose blossoms appear perfectly hardy, and capable of setting well in the open air; and the fruit of some of them is ripening a good deal earlier in the present year than that of either of the parent plants. The berries, however, are smaller than those of the chasselas, and with less tender and delicate skins: and, though not

* This grape is often confounded by gardeners, both with the white chasselas and white muscadine.

without

without considerable merits for the desert, they are generally best calculated for the press: for the latter purpose, in a cold climate, I am confident that one or two of them possess very great excellence. I sent a bunch of one of those varieties to the Horticultural Society, in the last autumn, and I propose to send two or three others in the present year.

Good for wine in a cold climate.

I have subsequently obtained plants from the white chasselas and sweetwater, the appearance of which is much more promising; and the earliest variety of the grape I have ever yet seen, sprang from a seed of the sweetwater, and the farina of the red frontignac. This is also a very fine grape, resembling the frontignac in colour and form of the bunch; but I fear its blossoms will prove too tender to succeed in the open air in this country; a single bunch, consisting of a few berries, is, however, all that has yet existed of this kind.

Very early variety.

The present season also affords me two new varieties of the vine, with striped fruit, and variegated autumnal leaves, produced by the white chasselas and the farina of the Aleppo vine: one of these has ripened extremely early, and is, I think, a good grape. When perfectly ripe, I propose sending a bunch of it for the inspection of the Horticultural Society.

Vine with striped fruit and variegated leaves.

In all attempts to obtain new varieties of fruit, the propagator is at a loss to know what kinds are best calculated to answer his purpose; and therefore, I have mentioned those varieties of the grape, from which I have propagated with the best prospect of success. My experiments are, however, still in their infancy; and I do not possess the means of making them on so large a scale, or in so perfect a manner as I wish: nevertheless, the facts of which I am in possession, leave no grounds of doubt in my mind, that varieties of the grape, capable of ripening perfectly in our climate, when trained to a south wall, and of other fruits, better calculated for our climate than those we now cultivate, may readily be obtained; but whether the mode of culture I have adopted and recommended be the most eligible, must be decided by future and more extensive practice.

Experiments yet in their infancy.

Better varieties may no doubt be produced.

I have made experiments similar to the preceding, on the peach; but I can say no more of the result of them, than

Peach.

that the plants possess the most perfect degree of health and luxuriance of growth, and that their leaves afford satisfactory evidence of the good quality of the future fruit. I am ignorant of the age at which plants of this species become capable of producing blossoms; but the rapid changes in the character of the leaves and growth of my plants, which are now in their third year, induce me to believe, that they will be capable of producing fruit at three or four years old.

May be brought to bear at 3 or 4 years old.

I shall finish my paper with stating a few conclusions, which I have been able to draw in the course of many years close attention to the subject on which I write.

Best mode of obtaining new varieties.

New varieties of every species of fruit will generally be better obtained by introducing the farina of one variety of fruit into the blossom of another, than by propagating from any single kind. When an experiment of this kind is made, between varieties of different size and character, the farina of the smaller kind should be introduced into the blossoms of the larger; for, under these circumstances, I have generally (but with some exceptions) observed a prevalence in fruit of the character of the female parent; probably owing to the following causes. The seedcoats are generated wholly by the female parent, and these regulate the bulk of the lobes and plantula; and I have observed, in raising new varieties of the peach, that when one stone contained two seeds, the plants these afforded were inferior to others. The largest seeds, obtained from the finest fruit, and from that which ripens most perfectly and most early, should always be selected. It is scarcely necessary to inform the experienced gardener, that it will be necessary to extract the stamina of the blossoms from which he proposes to propagate, some days before the farina begins to shed, when he proposes to generate new varieties in the manner I have recommended. When young trees have sprung from the seed, a certain period must elapse before they become capable of bearing fruit, and this period, I believe, cannot be shortened by any means. Pruning and transplanting are both injurious; and no change in the character or merits of the future fruit can be effected, during this period, either by manure or culture. The young plants should be suffered to extend

Peach.

Choice of seeds.

Seedling trees.

Should not be pruned or transplanted.

extend their branches in every direction, in which they do not injuriously interfere with each other; and the soil should just be sufficiently rich to promote a moderate degree of growth, without stimulating the plant to preternatural exertion, which always induces disease*. The periods which different kinds of fruit trees require to attain the age of puberty, admits of much variation. The pear requires from twelve to eighteen years; the apple, from five to twelve, or thirteen; the plumb and cherry, four or five years; the vine, three or four; and the raspberry, two years. The strawberry, if its seeds be sown early, affords an abundant crop in the succeeding year. My garden at present contains several new and excellent varieties of this fruit, some of which I should be happy to send to the Horticultural Society, but the distance renders it impracticable†.

Soil not too rich.

Age of bearing.

VI.

Memoir on the Desulphuration of Metals: by Mr. GUENIVEAU, *Engineer of Mines*‡.

AMONG the great number of metallic sulphurets, with which Nature presents us, the decomposition of many is of much importance in the arts. The sulphurets of iron, copper, lead, and mercury, and some others, give rise to metallurgical processes, that particularly claim the attention of those, who are addicted to the study of chemistry. The nature and properties of these have been well known, since chemistry has made them an object of her labours: but as the facts collected in laboratories have never been carefully

Decomposition of native sulphurets important.

The facts of the laboratory have not been compared with those of the smelting house.

* The soil of an old garden is peculiarly destructive.

† The hautboy strawberry does not appear to propagate readily with the other varieties, and may possibly belong to an originally distinct species. I have, however, obtained several offspring from its farina; but they have all produced a feeble and abortive blossom. If nature, in any instance, permits the existence of vegetable mules (but this I am not inclined to believe) these plants seem to be beings of that kind.

Hautboy.

‡ Journal des Mines, No. 121, p. 5.

compared

compared with those that extensive works furnish, though we are well aware, that this would be the best way of attaining useful results, the theory of the various operations to which sulphurets are subjected has not yet been improved by the progress of that science. My object is to supply this defect; and to accomplish it I have collected a great many experiments and observations, that have been long known; I have added some researches of my own; and from their examination I have deduced consequences, that must make some alteration in the ideas generally entertained respecting the treatment of metallic sulphurets.

Sect. I.

Of the action of heat on metallic sulphurets.

Heat always employ to decompose them;

THE action of heat on metallic sulphurets requires first to be examined, because it occurs in all the processes employed for their decomposition. To appreciate this with accuracy, I have chosen experiments and observations in which this action is completely distinct, which it is of importance to remark; for it is owing to not having analysed the effects produced by various causes, that metallurgists have ascribed to caloric alone a desulphurating power, which it does not appear to me to possess in any very high degree.

but it has not much effect alone.

Sulphurets of mercury and arsenic sublimed by it.

The sulphurets of mercury and arsenic are volatilized in close vessels, when exposed to a temperature a little elevated. The sublimed sulphuret has frequently a different colour from that which has not been sublimed; and the experiments of Proust and Thenard demonstrate, that this change is the consequence of an alteration in the proportions of the elements of the compound.

That of iron not freed from half its sulphur by it.

The native sulphuret of iron experiences but a partial decomposition by means of caloric. By distillation in a retort we cannot extract half the sulphur it contains. In Saxony the distillation of pyrites in the large way never yields more than 13 or 14 per cent of their weight of sulphur.

As these facts were not sufficient to determine my opinion respecting the effects of heat, because all the experiments that

that had come to my knowledge were made at no very high temperature, I put into a crucible lined with charcoal some powdered pyrites, covered it with charcoal powder, and exposed it for an hour to the heat of a forge. The result was a mass still retaining all the characters of pyrites. It appeared to have been completely melted, and retains two thirds of the sulphur originally belonging to it. This experiment, having been repeated, left me no doubt of the effects of heat alone on sulphuret of iron; and I think I may conclude from it, that, whatever be the temperature, only a partial decomposition can be produced by it.

Powdered pyrites, heated for an hour in a forge fire, was fused, and retained two 3ds of its sulphur.

On sulphuretted copper and pyritous copper heat produces effects analogous to those observed with iron. The distillation of pyritous copper afforded me but very little sulphur. These two ores however may be considered as mixtures of the sulphurets of copper and of iron, and the sulphur separated by heat comes from that of iron almost wholly.

Sulphuretted and pyritous copper similar.

The sulphuret of lead, or galena, is one of those minerals, the treatment of which is most varied. All chemists agree in considering it as a compound of sulphur and lead only, in the proportion of 15 parts sulphur to 85 of lead. I was the more careful in observing the effects of caloric on galena, as in separating the sulphur by its means I might hope to obtain metallic lead, the weight and fusibility of which would render its union very easy. I could likewise without difficulty exclude the air in the process.

Galena.

Into a retort I put 30 gram. [463 grs.] of powdered galena, which I heated for two hours so gently as not to agglutinate it. Only a little sulphurous acid produced by the action of the air of the vessels was evolved, and I perceived no sulphur sublime in the neck of the retort. I then increased the fire, and kept it thus two hours more, till both the galena and the retort experienced a commencement of fusion. The sulphur volatilized in this second stage of the operation was so little, that I could not detach it from the vessel and weigh it. The residuum had the metallic brilliancy, was agglutinated, and did not contain an atom of ductile lead.

Heated gently gave out a little sulphurous acid, but no sulphur.

Heated nearly to fusion

very little sulphur sublimed.

As the heat in this experiment was not very great, I subjected

Fused in a

forge fire scarcely 2-5ths of the sulphur expelled.

jected to a forge fire some powdered galena in a crucible lined and covered with charcoal powder. The result was a mass, that had been well fused, and resembled what metallurgists call lead matt. There was in it no lead united together, but some parts of the button were merely a little ductile. By analysis I found, that about three fifths of the sulphur still remained. Part of the loss it had experienced by the action of the fire, which was 27 per cent, I ascribed to the volatization of a portion of the sulphuret; for that owing to the separation of the sulphur could not have exceeded 6 per cent at most.

Loss ·27 from partial volatilization.

Galena then is but very imperfectly decomposed by heat.

I shall not speak particularly of the sulphurets of zinc, antimony, &c., because I am not acquainted with any experiments sufficient to determine with certainty the effects, that heat produces on them: but I am led to believe from analogy, that it does not decompose them completely.

Heat alone then expels but little of the sulphur.

All the facts I have adduced appear to me to evince, that the action of caloric alone on metallic sulphurets, and particularly on those of iron, copper, and lead, is limited to the taking from them a small portion of the sulphur contained, and afterward fusing and even volatilizing them.

Sect. II.

Of the simultaneous action of heat and air on metallic sulphurets.

Roasting by the joint action of heat and air.

That metallurgical process, the object of which is the desulphuration of metals, is known by the name of roasting. Most authors, who have treated of it, seem to consider caloric as the sole agent in the decomposition; and even those who have remarked the influence of the air, since the establishment of the new chemical theory, have not considered it as essential. The experiments I have collected having shown the insufficiency of heat alone to decompose a metallic sulphuret, the oxigen of the air must be considered as having a greater share in the desulphuration of metals by roasting. The affinities both of sulphur and metallic substances for this principle render it very probable; and it is likewise proved by the chemical examination of the products

Oxigen has a great share in it.

of

of all roastings, as well as by the manner in which the process is conducted. In the roasting of sulphurets, instead of seeing the volatilization of the sulphur effected by a moderate and long continued heat, we find a sulphuret decomposed by the simultaneous action of caloric and air: and the acknowledged necessity of not fusing the ore, instead of arising from the fear of communicating to it by liquefaction a cohesive force capable of resisting the separation of the sulphur, will be ascribed more simply to this circumstance, that such a state will confine the action of the air to a surface that cannot be renewed, and will soon be covered with a metallic oxide. The combination of oxigen with the elements of sulphurets gives rise to oxides and acids, the affinities of which have great influence on the separation of the sulphur, and the results of roasting; which are commonly a mixture of an oxide, a sulphate, and undecomposed sulphuret. I shall now examine more particularly and separately the roasting of several kinds of sulphurets, because the nature of the metal greatly modifies the results; and I shall afterward point out how the sulphur is separated, and in what form.

The ore must not be heated to fusion.

Roasting of copper pyrites.

Pieces of pyritous copper are laid on billets of wood in the most convenient manner for the combustion to continue a long time. The first heat separates part of the sulphur, which is in some degree sublimed, and may be collected; but afterward it becomes the combustible, that serves by burning to continue the operation; and sulphurous acid is disengaged, the elasticity of which, being augmented by the increase of temperature, prevents its combination with the metallic oxides. The sulphuric acid, that is formed notwithstanding the care taken to moderate the combustion, unites with the oxides of iron and copper, but the sulphate of iron is in part decomposed by the superoxidation of the metal.

Copper pyrites. Sulphur rises, burns, sulphurous acid flies off, sulphate of copper formed, and of iron which is again in part decomposed.

Iron pyrites subjected to the same operation will undergo similar decompositions in the same order.

Iron pyrites.

The roasting of cupreous pyrites in the reverberatory furnace gives rise to the same phenomena, and might be supposed

Copper pyrites in the reverberatory furnace.

posed to allow a much more complete separation of the sulphur than that conducted in the open air. If it do not, this no doubt is owing to the difficulty of preventing the agglutination of the sulphuret produced by the elevation of temperature arising from the rapid and unavoidable combustion of a large quantity of sulphur.

Furnace at Fahlun.

It remains for me to speak of a furnace, in which both the smelting and roasting of the pyritous copper, to a certain point, are effected at the same time. It is used at Fahlun in Sweden. This has an interior crucible, which receives the product of a smelting of 24 or 48 hours, and in which a separation, or rather combustion, of the sulphur is effected. A stream of air from the bellows is made to blow on the melted mass with such force, as to drive off the scoriæ, and burn a part of the sulphur that is found on the surface. The iron is thus oxided, and quartz is added to vitrify it in proportion as the roasting goes on. This process is perhaps the only one, in which sulphur and iron are separated in so large a quantity at the same time.

Blast of air directed on the metal.

Progress of desulphuration.

The desulphuration of pyritous copper by roasting appears to me to be effected, 1st, by the sublimation of a small portion of sulphur, which may either be collected, or burned in the air: 2dly, by the disengagement of sulphurous acid, which is the more abundant in proportion as the process is well managed: 3dly, by the vaporization of a little sulphuric acid, the greater part of which however remains united with the copper.

Roasting of galena.

Sulphuret of lead.

Galena is very difficult to desulphurate completely by roasting. The affinity of its component parts for oxigen, it is true, renders their separation sufficiently speedy; but that of the new compounds, sulphuric acid and oxide of lead, gives rise to a new combination, which retains the sulphur, and thus forms an obstacle to the desulphuration. To this affinity of the oxide of lead for sulphuric acid must be ascribed the facility, with which this acid is formed in the roasting of galena.

I shall analyse in detail the various processes, to which this important decomposition has given birth, because I conceive

ceive I can account for the numerous and complicated phenomena they exhibit.

Whatever care be taken to roast galena in a roasting test, it is impossible to convert the whole of the sulphur into sulphurous acid, and avoid the formation of sulphuric. The result always exhibits a mixture of oxide and sulphate of lead. In the small way sulphate always formed.

In roastings in the large way, on hearths prepared for the purpose, the proportion of sulphate of lead is still more considerable, being in the ratio of the temperature, and the facility with which the air pervades the ore. Numerous analyses made in the School of Mines lead me to believe, that the roasted ore of the mines at Pezey contains from a third to half its weight of sulphate of lead; whence it follows, that, even supposing the whole of the galena to have been decomposed, the roasting has not separated half the sulphur it contained. In the large way still more sulphate.

The reverberatory furnace is employed with great success to roast ores of sulphuretted lead. In some works indeed, as at Poullaouen, such a complete separation of the sulphur is accomplished in this furnace, that, when the roasting is judged to be finished, nothing more than the addition of charcoal is requisite, to obtain directly a large quantity of metallic lead. It cannot be doubted however, but a great deal of sulphate of lead is formed, which, as we have seen, is a necessary result of the action of air on galena subjected to a high temperature: besides, the chimneys of the furnaces are filled with it. The decomposition of this sulphate by the charcoal produces a sulphuret, or lead matt, and though sulphurous acid may be evolved, it is very difficult to explain how the addition of charcoal causes the lead to flow immediately in considerable quantity. I have imagined that the sulphate of lead was decomposed during the roasting; and that after this operation nothing remained but an oxide very little mixed: and I think I have found the cause of this decomposition in the action of the galena still undecomposed on the sulphate formed. The following experiments will make known the nature and result of this action. In the reverberatory furnace a complete separation of the sulphur may be effected. Yet sulphate is formed.

Into a retort I put a mixture of one part of powdered sulphuret of lead and three of sulphate; which at first I heated slowly, Sulphuret 1 part & sulphate of lead 3 heat-

ed together in a retort. slowly. When the retort was redhot, a pretty considerable quantity of sulphurous acid gas was evolved; and this continued for an hour, at the expiration of which the retort began to melt. The residuum had been fused, and was found to be a mixture of oxide and sulphate of lead. I satisfied myself, that the sulphurous acid, which had been received into water, was not mixed with any sulphuric.

The result a mixture of oxide & sulphate.

The sulphate decomposed by the sulphuret.

This experiment proves the possibility of the decomposition of the sulphate of lead by the sulphuret; or rather that of the sulphuric acid it contains by the sulphur and lead of the galena. The sulphurous acid arises no doubt equally from the oxigenation of the sulphur, and the semidecomposition of the acid; for I convinced myself, that the residuum contains no sulphate. I repeated the process with equal parts of galena in sulphate, when the evolution of sulphurous acid was still more abundant, and what remained in the retort was a mixture of oxide and sulphuret. Hence I concluded, that, if the proportion of sulphuret of lead were too small in the former experiment, it was too large in this. I made also an attempt to ascertain more nearly the proportions, that would exactly effect the mutual decomposition; and at the same time I endeavoured to satisfy myself of the oxidation of the lead contained in the galena in the metallic state.

Equal parts left oxide and sulphuret.

14 parts to 8.

With this view I put 14 gram. of sulphate and 8 of sulphuret, well mixed together, into a crucible, not lined, which I suffered to grow redhot undisturbed. I observed, that a considerable ebullition took place, occasioned by the evolution of sulphurous acid; and I did not withdraw the crucible, till the matter was in quiet fusion. When cold I found two distinct substances; one, which was at the bottom, consisted entirely of sulphuret of lead, that had been fused, without any mixture of ductile lead; the other exhibited all the characters of the oxide called glass of lead, and was a compound of oxide and silex from the crucible, without any indication of sulphate of lead.

Sulphurous acid evolved.

Result, sulphuret and glass of lead.

This experiment convinced me, that the lead of the galena had been oxided at the expense of the sulphuric acid: but it did not show the quantity of galena necessary for the complete decomposition of the sulphate. I believe, however,

Sulphuret 1

ever, that the proportion of one part of galena to two of sulphate will be very near the mark; and besides it differs little from what a calculation of their component parts would indicate. sulphate 2 would produce a decomposition.

The following are the natural consequences of these facts: 1, Galena and sulphate of lead mutually decompose each other at a high heat. 2, This decomposition gives rise to the formation and evolution of a large quantity of sulphurous acid, and consequently to the separation of a considerable portion of the sulphur contained in the ore. 3, The result is oxide of lead, when the proportions are suitable; and when otherwise a mixture of oxide and sulphate, or oxide and galena. Conclusions.

The application of these consequences to the roasting of sulphuret of lead in the reverberatory furnace is easy. I shall explain the theory of this process in the manner in which I conceive it. The powdered galena, or washed ore of lead, spread on the bottom of the furnace to the thickness of a few inches, the upper part of which is exposed to the action of the air, gives rise to the phenomena we have observed in ordinary roastings. The heat vaporizes a little sulphur: the air converts part of that on which it acts into sulphurous acid, which is evolved, and another more considerable into sulphuric acid, which combines with the lead oxided at the same time. The ore is stirred: the sulphate of lead mixes with that which is not decomposed, and their mutual decomposition produces sulphurous acid: the fresh surface reproduces sulphate, which serves to occasion a fresh extrication of gas, and thus to continue the desulphuration, which is limited only by the complete decomposition of the galena. If the process have been well managed, and too much sulphate of lead has not been formed, the result of the roasting will be almost pure oxide of lead: if the contrary, some sulphate may remain, which the charcoal will reduce to the state of sulphuret, and the decomposition of which will be effected in the same way as that of so much galena. Hence we may learn the importance of not fusing the sulphuret of lead subjected to the process of roasting; for the action of the air on the fused ore would soon be rendered null by the formation of oxide of lead which would cover it, Theory of roasting galena in the reverberatory furnace.

and

and as the sulphate of lead could no longer mix with the galena, there would be no way of desulphurating it.

This the most complete desulphuration.

The roasting of galena in the reverberatory furnace then is reduced to the conversion of the sulphur it contains into sulphurous acid; and as this is in great measure effected by the intervention of the sulphate of lead, which is continually forming, it admits a more complete desulphuration than other processes.

Decomposition in the Scotch furnace similar.

A similar decomposition of the sulphuret of lead by the sulphate appears to me also to take place in the treatment of lead ores in the Scotch furnace. In Scotland galena is roasted and smelted in an uninterrupted process by means of coal and turf.

Used with success at Pezey.

The same furnace is employed with success at Pezey for fusing roasted galena containing at least one third of its weight of sulphate of lead. Its final result gives no matts, which proves, that it permits the decomposition of the sulphate, and the separation of the sulphur it contains. I conceive, that the action of the part reduced to the state of sulphuret, by the contact of the charcoal, on the undecomposed sulphate, is one of the principal causes of the desulphuration effected.

In some furnaces scarcely any effect from roasting.

Some furnaces have been mentioned, as that of Fahlun and the Scotch, in which metallic sulphurets undergo a real roasting; but there are others, in which this effect is scarcely sensible. Some reflections on their differences in this respect will probably not be out of place here; and they will be the more interesting, as they are intimately connected with our subject, and account for phenomena, which are inexplicable according to the idea generally entertained of roasting.

The higher the furnace the less sulphur carried off.

It is a fact well known in smelting houses, that the highest furnaces are least favourable to desulphuration, or in the language of metallurgists produce the most matts. If an indisputable proof of this were required, I need only say, that at Pezey I have seen roasted lead ores containing a great deal of sulphate of lead, which smelted in the Scotch furnace yielded not matts as the ultimate result, but produced a large quantity in the *fourneau à manche* [a kind of high furnace].

Heat alone ineffectual.

If heat alone could easily and completely decompose metallic

fallic sulphurets, the upper part of high furnaces would be well adapted to the roasting of ores; for, beside that the temperature there is not too great, the air that comes thither, being deprived of part of its oxigen, scarcely forms any of those sulphates, that oppose the separation of the sulphur. But the fact is the reverse of this, which is to me an additional proof of the little effect of the action of caloric alone on these substances. The sulphur is separated from the sulphurets, as has been seen, in the state of sulphurous acid, and oxigen is indispensable to its formation. In furnaces of no great height the air that comes into contact with the fresh charge of ore still contains a great deal of oxigen, and the sulphurous acid formed is soon withdrawn from the disoxiding action of the charcoal: but if a small portion be decomposed, a fresh sulphuret is formed, which is afterward roasted in the same manner as the ore. In the Scotch furnace for instance, when any matts flow from it, they are immediately thrown into the furnace again, and what escaped decomposition in the first process is decomposed in a second. In high furnaces on the contrary the ore placed in the upper part undergoes a very incomplete desulphuration, because the air coming into contact with it contains but very little free oxigen; the sulphurous acid formed in the interior is far the greater part decomposed in traversing all the height of the furnace filled with coals, and a sulphuret is recomposed; this by its gravity tends to descend into the basin, which it does not reach till after a succession of decompositions; and the consequence must be a considerable loss of metal, as in fact is observed.

Advantages of a lower furnace.

Disadvantage of the higher.

All these facts together seem to me to place it beyond doubt, that the decomposition of metallic sulphurets in roasting is produced by the oxigenation of their component parts, and the sulphur is separated more or less completely in the state of sulphurous acid.

These proofs of the utility of oxigen.

SECT. III.

Desulphuration of metals independently of the action of the air.

The various affinities of sulphur for different mineral substances afford means of decomposing certain sulphurets, and metallurgists have already availed themselves of several with success.

Desulphuration by elective attraction.

Requisite conditions.

success. In order that the decomposition of a metallic sulphuret by any mineral may constitute the basis of a metallurgical process, it is not sufficient, that the affinity of this mineral for sulphur be greater than that of the metal: it is farther necessary, beside the conditions economy requires, that several others indispensable to the success of the process be satisfied, which greatly diminishes the number of agents indicated by chemistry. For example, if the sulphuret resulting from the decomposition be infusible, or nearly so; or if it have the property of combining with the metal to be separated, or with the sulphuret yet undecomposed; it is obvious, that the object sought, which is the separation of the metal, will not be obtained. Hitherto scarcely any thing but lime and iron has been employed.

Desulphuration of mercury.

Sulphuret of mercury by lime or iron.

The sulphuret of mercury is easily decomposed. It is sufficient to present to the sulphur a substance capable of retaining it, and the mercury may be volatilized alone. Thus iron and lime are employed singly or conjointly in the treatment of cinnabarine ores.

Desulphuration of copper.

Copper pyrites by lime.

Copper pyrites are smelted in some works with lime, either in the *fourneau à manche*, or the reverberatory furnace; but this process is not sufficiently known in detail, to enable us to judge of the efficacy of this agent.

Iron does not answer.

I had thought with some metallurgists, that the acknowledged greater affinity of iron than of copper for sulphur might occasion the decomposition of sulphuret of copper by this metal, at least in some cases: but the experiments I am about to give induced me to relinquish this opinion.

Experiment in proof.

1st *Exp.* I mixed 10 gram. [155 grs.] of pyritous copper, the composition of which I knew, with 4·3 gram. [66 grs.] of iron filings; put the mixture into a crucible; covered it with charcoal powder; and heated it in a forge fire three quarters of an hour. The proportion of iron was calculated so as to be sufficient for taking up all the sulphur combined with the copper in the ore employed. In the crucible I found a perfectly homogeneous mass, weighing 13·1 gram. [202 grs], which did not contain the least globule of metallic copper,

nor

or any sign of separation between the sulphuret of iron and that of copper*.

2d *Exp.* Another trial was made with 10 gram. [155 grs] of pyritous copper and 5 gram. [77 grs] of the same mineral roasted, which is nearly the state of the product when the ore or matts have not been completely desulphurated. The proportion of iron was still insufficient to separate any copper, of which there was abundance in the mixture. I heated it three quarters of an hour, and found, as in the preceding experiment, a homogeneous mass, without any sign of metallic copper, or pure sulphuret of copper: it was a true copper matt. 2d experiment.

3d *Exp.* Equal parts of crude and roasted copper pyrites were mixed, moistened with olive oil, and heated strongly for half an hour in a crucible lined with charcoal. The product was nothing but a powder, that had not undergone any fusion, no doubt owing to the superabundance of iron. 3d experiment.

These few trials I conceive are sufficient to prove, that the desulphuration of copper by means of iron will always be very difficult to effect, because a triple compound of sulphur, iron, and copper, is formed, or a combination takes place between the sulphurets of copper and iron, which obstructs the separation of the copper. Iron, sulphur, and copper, from a triple compound in any proportion.

Desulphuration of galena.

Galena is one of those sulphurets, in which this decomposition is most readily effected. The fusibility of lead, which facilitates the union of its particles, as well as the little affinity it has for sulphur, are the causes of the success of the attempts of this kind. Lime and iron are employed in different circumstances for the desulphuration of galena. The use of lime is not very general, and it is impossible to judge of its effects from what is known of the properties of sulphuret of lime. The treatment of galena by malleable or cast iron in small pieces is more in use, and appears very advantageous. Sulphuret of lead. by lime and iron. Lime little used. Iron preferable.

* In the decomposition of galena by iron, when the latter is in too small quantity, three distanct substances may be observed: lead, sulphuret of lead, and lastly sulphuret of iron at the upper part.

Experiments at the School of Mines.

At the school of mines of Montblanc a great many experiments have been made on the desulphuration of galena by iron, the results of which were of sufficient importance, to render the publication of them desirable.

Hints may be drawn from the facts here given.

The present paper contains several facts applicable to the art of metallurgy, and capable of suggesting different experiments to those who cultivate it. I have not pointed out any, because they will readily suggest themselves to those who are capable of conducting them.

Mr. Descotils superintended the experiments.

All the experimental researches here given were made in the laboratory of the Council of Mines, and under the eye of Mr. Descotils, whose advice was of great advantage to me, in giving them that accuracy, which he is accustomed to observe even in the least operations.

VII.

Heights of various Places determined by the Barometer, in the Course of several Tours through France, Switzerland, and Italy: by F. Berger, *M. D., of Geneva**.

Ascertaining heights of places tends to the improvement of geology. La Place.

AMONG the means best calculated to advance the physical department of geography in the present state of our knowledge we may reckon the ascertainment of the elevation of a great number of points on the Earth's surface. The learned author of *la Mécanique céleste* has proposed to employ with this view observations with the barometer conjointly with the longitude and latitude, to obtain a more complete and extensive levelling than trigonometrical measurement will admit, and at the same time to acquire a knowledge of the direction of mountainous chains, the slope of rivulets, and the forms of countries. To promote these useful objects I shall add to the researches of these natural philosophers, who have attended to this branch of physics, the observations I have collected in different journies. They will form the subject of a few papers, which I shall publish

* Abridged from the Journal de Physique, vol. LXIV, p. 220, March, 1807.

in

a succession, and in which I shall point out generally the nature of the countries mentioned, that my labours may be more immediately useful to geology.

All the heights were calculated according to the formulæ of Messrs. de Luc and Trembley: not that I mean to speak of them as the only ones fit for practice; I know there is a method founded on different principles, which was proposed some years ago by a learned philosopher, and has a just claim to our best attention: but my labours were in great measure completed, before I was acquainted with it, so that it would have been too laborious a task, to begin them anew.

Heights calculated according to de Luc and Trembley.

They who have at heart the improvement of measuring heights by the barometer however would do well, to calculate their observations according to different formulæ, for on this point the result of experience is chiefly to be considered, and we have not yet a sufficient number of decisive facts, to induce us to employ one formula or method exclusively of every other. It is desirable that some philosopher, residing in the neighbourhood of a solitary mountain, should measure it trigonometrically with great exactness, and afterward repeatedly ascertain its height by the barometer, calculating his observations according to several formulæ, and comparing their results with his geometrical measurement. It is particularly important, that these observations should be made at all seasons of the year, and at different times of the day, so as to take in every variation of temperature: but in reallty when mountains of moderate height are visited, we generally find on them the same degree of temperature, and most frequently that at which the various formulæ give nearly the same results. We ought not to suppose therefore, that the science has already attained such a degree of perfection, as to enable us to dispense with farther trials: the only course to be followed appears to me to be that of experience.

Modes of ascertaining heights by the barometer should be compared with trigonometrical calculations;

and tried at various seasons and different times of the day.

The necessity of employing good instruments is too obvious to mention. It is likewise to be wished that the traveller, into whose views it may not enter to give heights with any great precision, as the geologist in general for instance, would apprise us of this. It would farther be of advantage,

When calculations are made in a loose way, this should be mentioned.

when accurate barometrical measurements are published, to mention the hour of the day when they were made; and at the same time the measurements of other travellers, however different they may be from our own, with the formulæ they employed if possible. Thus useful materials would be stored up for the promotion of science.

The following observations are classed according to the journies in which they were made; and in them I have conformed as much as possible to the rules I have laid down.

Sect. I.

Tour through Picardy and Normandy.

Heights ascertained during a tour made in the ci-devant *provinces of Picardy and Normandy.*

This tour was made toward the end of the summer of 1803, in company with my friends and colleagues Messrs. Jurine and de la Roche. I set off from Paris with intention to follow the seacoast as much as possible, to examine the structure of the shores, and the different elevations of the cliffs. I used a siphon barometer made by Dumotier, which however I did not think sufficiently accurate to allow me to consider my results in any other light than simply geological.

Its leading objects.

Chalk soil interspersed with flints.

It is well known, that part of the soil of France, proceeding in a north-west direction from Champagne to the borders of the sea, is composed of chalk, which continues as far as England, and includes flints of irregular form, separate from each other, but arranged in parallel beds, more or less distant, which alternate with the chalk. Beauvais, Amiens, &c., are in this line. On the left bank of the Somme, below Amiens, are little hills of no great height formed of this chalk, which is burned for lime. At Picquigny there is very good peat.

Stratum of flints, which when washed out form the pebbles on the shore.

At St. Valery on the Somme the cliff is not above 60 or 80 feet high. The chalk is in horizontal strata, a foot and half thick, between which is a very thin bank of flints. These flints, separated and rounded by the waves of the sea, appear to compose the pebbles that are found at the mouth of the river. At Crotoy, a town formerly fortified, and built on the right bank of the Somme at its mouth, the cliff

no

no longer exists, and we find only a white quartzose sand, forming downs of little height following the direction of the coast toward St. Quentin. The same sand is seen all along the coast from St. Valery to Cayeux, and even to the environs of the town of Ault. This alluvion therefore, if it have been produced by the Somme, occupies a space of at least 7 or 8 leagues [18 or 20 miles]. Is it not rather owing to the retreat of the sea?

White quartz sand,

perhaps left by the retreat of the sea.

Near the town of Ault the pebbles are so accumulated on the borders of the sea, that they extend above a mile inland. I found there a flint passing to the state of calcedony. At this town the cliffs reappear, still exhibiting the same structure. At the city of Eu there is an interruption of the cliffs, which appear again at Treport with the same character.

Extensive bed of pebbles inland.

From Criel to Dieppe the soil is essentially sandy. This town is in a bottom, through which the river Arques flows: and it is these valleys, watered by so many different rivers, the course of which in general approaches more or less a west-north-westerly direction, that the continuity of the cliffs is interrupted. From Fécamp to Hâvre, the country being less intersected by them, the cliffs are more continuous.

Sandy soil.

Cliffs interrupted by rivers.

At cape de la Hève, about a mile and a quarter north-north-west of Havre, the cliff is not so abrupt as at Ault and Treport; in other respects its structure is nearly the same. At the bottom, toward the village of St. Adresse, a bank of marle is found, of which bricks are made: and the chalk rock includes different kinds of petrifactions, as well as flints, and nodules of pyrites, which are decomposed by oxidation when exposed to the air. The cliff is continued up the course of the Seine: and at Orcher, a pleasant village 3 leagues [7½ miles] east of Havre, it is about 200 feet high. Here it is more abrupt than at cape de la Hève, and about a fifteenth part at the bottom is composed of a sandstone with small siliceous pebbles.

Marle.

Petrifactions and pyrites in the chalk.

Sandstone under it.

At Honfleur the cliff ceases to contain strata of flint, and diminishes in height as it approaches the mouth of the Toucques, 4 leagues [10 miles] farther. From Tronville sur mer to within 4 miles of the mouth of the Dives it almost

Flints cease.

Reddish, shelly, limestone.

most wholly disappears. A reddish, shelly calcareous stone is very abundant on the shore. Imperceptibly the cliff rises again, and opposite the rocks called the Black Cows it is about 150 feet high, about two thirds of the lower part, being a blueish marle, include a large species of fossil oyster, called the crested oyster, other petrifactions, and signs of bituminized wood. The upper part is chalk.

Blue marle including fossil shells, and traces of bituminized wood.

Sand.

From Dives to Savenelles, or Sallenelles, at the mouth of the Orne, nothing appears but sand, forming in some places downs. On proceeding up the Orne, near a mile and half beyond Savenelles, are quarries of a large grained calcareous stone, soiled with yellow earth, lying in horizontal strata, and used for building in the country round.

Caen river building stone.

From Gray, at the mouth of the Seule, no cliff is seen till we come to Tracy, a village 8 or 10 miles to the west-south-west. There it is about 200 feet high; and is formed of a very fine grained blueish freestone, tolerably hard, interspersed with scales of mica, lying in horizontal strata, and including a prodigious quantity of cornua Ammonis, some of which are very large. The whole of this coast abounds in fuci and other marine plants.

Cliff of blueish freestone.

Cornua Ammonis.

Fuci.

The same calcareous freestone forms the substratum of the soil from Bayeux to Littry, a village 5 miles to the south-west. At Littry is a coalpit, that deserves the attention of the naturalist. It was opened in 1741, and has two shafts, one of which, called St. George's, is 345 feet [368 Eng.] deep, and has several extensive galleries issuing from it. The thickness of the coal varies from $4\frac{1}{2}$ to 9 feet [4 feet 10 in. to 9 feet 7 in.]: it lies on a brownish calcareous freestone, but little effervescent; and this on a clay, very soft to the touch, and not attackable by acids. At 250 feet [267 Eng.] from the surface occurs a stratum of a primitive conglutinated stone, composed of siliceous pebbles generally an inch or two in diameter, nodules of steatite, and thin laminæ of coal, cemented by a finer freestone, which does not effervesce with acids. No petrifaction has yet been found in this coal mine, except one branch of a tree, in which traces of woody fibres are perceptible. The coal is in general very sulphurous: that of the best quality is sold on the spot for

Coalpit.

26s. [13d.] the bushel, weighing about 130 lbs; and the worst fetches 15s. [7½d.]. The water of a well 18 feet deep, at the bottom of St. George's shaft, freezes on its surface in winter. On the 27th of September Deluc's thermometer stood there at 12·8 [60·8 F.], while in the open air it was at 17·6 [71·6 F.]. The water of this well is extremely acid.

Water at the bottom freezes in winter.

Cold greater than in the open air.

The Vire, which falls into the sea not far from Isigny, forms a considerable bank of sand at its mouth. There is no appearance of cliff here; but at Vierville, a small town on its left bank, and not far from the sea, there are some traces, which soon give way to the sands and downs, that extend to Ravenoville 7½ miles N. N. W. Throughout this space an immense quantity of shells are found, which are collected and sold to the farmers for dressing their grass land. The zostera marina, which covers the shore, is collected for the same purpose. The rudiments of cliff seen at Vierville consist of horizontal strata of free stone, alternating with clay; both including many petrifactions, particularly gryphites and ammites.

Sand bank.

Shells and zostera marina used as manure.

At Ravenoville, which lies opposite the isles of St. Marcouf, we enter at once upon the primitive class of stones. A beautiful kind of reddish scaly petrosilex forms the transition from the primitive substratum of the peninsula of Cherbourg to the shelly calcareous stone of the surrounding country. The houses of the neighbouring villages, as well as the forts on the coast of la Hougue, are built with this petrosilex.

Petrosilex.

The islands of St. Marcouf are probably of a similar rock, since the corresponding coasts of England are; so that, as Mr. Delametherie observes, we can scarcely doubt, that the granite extends far into the sea on both sides of the channel: and if it were ever to be laid dry, we should probably find the continuation of the granitic chain from one country to the other; or at least they would be separated only by a few plains of secondary formation, as are the granites of Britanny and what was formerly Limousin.

Granite probably extends under the sea across the channel.

Ravenoville is perhaps the most northerly place in France, where salt is made by imitating to a certain point the process

Salt pans.

cess of salt marshes: the tide flowing into basins formed in the sand, where the water stands some time to evaporate before it is boiled down.

The cliffs do not reappear as far as fort de la Hougue, where I was obliged to give up my design of doubling cape Barfleur, viewing the real granite in its native situation. A disagreeable event, which it would be useless to mention, obliged me to proceed directly to Valogne. On this road I continually met with argillaceous schist, which as it proceeds in land forms a series of woody hills, rising in height as they recede from the coast. Near Valogne the *ulex Europæus*, furze, is seen in abundance. It is sown there, to be burned on the land as a manure.

Schist.

Furze sown to be burned for manure.

Woody country.

From Valogne to Cherbourg the country is woody, and the soil reddish. Cherbourg is built on a substratum of light green steatite, very greasy to the feel, in laminæ more or less curved. In some places they are in separate pieces, coarse grained, and easily broken. The new basins for the harbour are cutting out of this rock. Large nodules of true granite, and veins of quartz, are included in the steatite.

Steatite.

Harbour of Cherbourg.

The mountain of Roule, a little to the south-south-east of Cherbourg, may be considered as constituting the cliff. It terminates abruptly toward the town in a precipice about 40 toises high. It consists of a kind of dull petrosilex, with a shelly fracture, in some places reddish, in others whitish, much like that of Ravenoville, but evidently in strata several feet thick, all running S. S. E. and N. N. W. Quartz crystals are occasionally found in it. This rock is wrought for the works of the harbour, being blasted in large masses.

Petrosilex.

Quarried for the harbour of Cherbourg.

Hilly country.

Between Cherbourg and St. Lo the soil is variable. Thence to Aulnay and on to Falaise it is hilly, and woody, but the trees are in general low. The usual direction of these hills is north and south, and they diminish in height as they approach the coast. They consist of a micaceous argillaceous schist, which does not effervesce with acids, and includes no organic bodies. In some places it passes into true slate. Near Villers however, in the district of Caen, is found a granulous limestone containing a prodigious quantity of belemnites. The furze on the downs is employed for feeding cattle.

Schist.

Limestone.

Cattle fed on furze.

The

The country on the right bank of the Orne begins to differ perceptibly from that on the left. At Ussy 10 or 11 miles N. N. W. of Falaise, limestone occurs in strata. The course of the Orne indeed appears the boundary of two different kinds of country: on the left bank we find micaceous argillaceous schist, and on the right limestone. The argillaceous schist of the woody part of Normandy may be considered as forming the transition to the primitive rock, that constitutes the most advanced part of the peninsula of Cherbourg to the N. N. W. At Verneuil nodules of flint reappear in the chalk, and we begin to perceive vineyards. The line traced by Mr. Arthur Young on this point appears to me very accurate.

Limestone strata

divided from the schist by the Orne.

Flinty chalk, and vineyards.

Thus we see, 1st, that the part of France where we find a chalky soil interspersed with flints stretches S. E. and N. W., and is pretty accurately included between the mouths of the Seine and the Lys, occupying a breadth of about 50 leagues [125 miles] and a length of 70 [175 miles]: 2dly, that in this the highest cliffs occur, at least among those that are seen between St. Valery on the Somme and Cherbourg. The following table will show this more conspicuously.

Extent of this soil.

Table of heights above the level of the sea.

Places.	Heights above the sea in toises & thousandth parts. According to Deluc.	According to Trembley.
Beaumont-sur-Oise	39·352	39·514
Amiens	38·801	38·977
Frixecourt	30·009	30·321
Ault	25·487	26·100
Tréport	59·458	60·999
Etretat	52·944	54·243
Cape de la Hève	46·545	47 729
Honfleur	41·798	42·887
Caumont	141·326	143·707
Cahagnes	88·206	89·135
Aulnay	58·617	58·913
Harcourt	22·672	23·229

VIII.

A new Method of Classing the Hymenopterous and Dipterous Insects: by L. JURINE, *Correspondent of the Institute, Professor of Anatomy, &c*.*

Hymenoptera a natural order.

Genera distinguished arbitrarily,

or by the parts of the mouth, which is difficult.

THE distinction of the order hymenoptera, pointed out by Aristotle, is so natural, that it has been retained in every system of entomology to the present day. Linneus, Geoffroy, and Degeer, divided it into a few genera, more or less arbitrary, from various particularities of confirmation: while Fabricius and Lareilie have attended in this point to the parts of the mouth. The difficulty of dissecting this organ however in the smaller species is a great inconvenience: to avoid which, and at the same time adhere more closely to the system of classification by the wings, Mr. Jurine has recourse to the disposition of the principal ribs of the wing for the generic characters.

New method by the ribs of the wings.

Having observed, that these ribs, by intersecting or terminating in each other, form various reticulations, which are constantly uniform in insects of the same kind, he has studied these systematically, and given accurate representations of those of the hymenoptera in 14 coloured plates, included in a quarto volume, in which he details his method. On the outer edge of the upper or larger wing of the hymenoptera are two large parallel ribs, appearing to issue from the corselet, and strongly united by an expansion of the membrane. The outermost of these he terms the *radial* rib, the innermost the *cubital*. The place where they terminate toward the end of the wing, which is commonly distinguished by a spot or mark more or less deep, he calls the *point*, or *carpus*. The rib that proceeds from this point to the extremity of the wing has a membranous space between it and the outer edge of the wing, forming one or more areas, which he names *radial cells*. From the extremity of the cubital rib, and near the carpus, another prominent line proceeds towards the extremity of the wing, and the interval between this and the

* Abridged from the Magazin Encyclopédique for April, 1807, p. 434.

preceding

preceding is the *cubital cell*, which is commonly divided into two, three, or four.

All these cells exhibit a great many differences: thus they are incomplete, appendiculate, petiolate, &c. These differences constitute the characters.

The whole of the hymenoptera with which Mr. Jurine is acquainted, and his own collection contains 2200 species, he includes in 86 genera, which he arranges in three suborders, distinguished by the manner in which the abdomen is attached. The diptera, arranged according to the same method, will shortly appear.

Divided into 3 suborders, and 86 genera.

The diptera promised.

IX.

Description and Manner of Using Mr. Robert Salmon's *Geometrical Plotting Quadrant, Level, and Calculator, for the Use of Navigation and Land-Surveying; ascertaining inaccessible Distances; and Demonstrating and Determining various Problems in Geometry and Trigonometry**.

On the instrument and parts thereof are engraved the names given by the inventor, and made use of in these explanations; the *base* line being that at right angles with the 90 degrees on the *arch*, as it is also to the *perpendicular*, which perpendicular always moves parallel to the 90 degrees. For the use of land-surveying, where the instrument can be made stationary, the sight (marked *a*, *Fig.* 1, *Pl.* V.) with the small hole in it, must be applied; but for sea-service, the one *b*, *Fig.* 2, with the mirror, must be substituted in its place.

Terms used.

Every person who has had occasion to describe or calculate the parts of the right-lined figures used in geometry, perspective, surveying, navigation, dialling, architecture, &c., must have perceived, that all of them are resolvable into the most simple of figures, a *triangle*, or some number of them.

All right-line figures resolvable into triangles.

Hence the great importance of geometry and trigonometry,

Use of trigonometry.

* The Society of Arts voted Mr. Salmon their silver medal and ten guineas for this invention. See their Trans. vol XXIII, p. 290, whence this article is extracted.

metry, in teaching, either by construction or calculation, the knowledge of all the properties or relations between the three sides and three angles, of which every plane triangle is composed. Euclid having demonstrated, in the fourth proposition of the sixth book of his Elements, that in any two *similar triangles* (by which he means their having the same angles, without regard to the actual lengths of their sides, for one triangle may be very small and the other ever so large) every pair of the corresponding sides in the two triangles are proportional; it is the business of trigonometry to solve such problems, with the help of the tables of sines and tangents, or of sectors, sliding or other rules, and scales, by which you can find, on inspection, a *right-angled* triangle, exactly similar to any given right-angled triangle, (or having one of its angles equal to 90°) which can be proposed, or can occur in practice; and by the Rule of Three we say, *as* any side of the tabular triangle *is to* the similar side, supposed to be known, of the triangle under consideration, *so is* any other side of the same tabular triangle, *to* the corresponding side supposed to be sought, of the triangle in question. It is evident, that by means of the *base line*, *perpendicular*, and either the *upper*, or *lower limb* of my instrument, by the two motions of which the *perpendicular* is capable, and the angular motion of which the *limbs* are capable; any right-angled triangle whatsoever, as C B E, or C D E, in the diagram *Fig.* 6, *Pl.* V, may be instantly formed, (by bringing the top corner of the *perpendicular* to touch the *limb*) with the same or greater facility, than it could be taken out of a trigonometrical table, measured by the compasses on the sector, or set on any instrument now in use for that purpose. But no instrument that I have seen or read of is capable of forming immediately *any obtuse-angled triangle*, as on my *geometrical plotting quadrant* can be done; nor can the trigonometrical tables be applied, to produce the sides and angles of such a triangle, without some trouble, in any case; and in some of the most useful cases in practice, the labour is very considerable. I shall therefore give the solution of five problems. First, supposing, that *Figure* 6, *Pl.* V, represents my instrument, set to answer this and the following problems; A, B, C, being the triangle under consideration;

Similar triangles proportional in all their parts.

Practically applied to rectangles.

Instrument applicable to these;

and equally to obtuse angles.

Problems to be solved by it.

ation; then since the ∠ A C E, is by Euclid (I, 20) equal to the ∠ B A C, it is evident that this angle will be shown, or may be set, by means of the divisions on the arc F G; also, that since C B E, and I C B, are also equal, the *arc* H I, with the addition of 90° (for the angle E B A), will show the ∠ C B A, of the triangle; it is equally evident, that the *arc* F H will show the sum of the two ∠s B C A, and A C F, at the same time that the lengths of all the sides may be read off, on the divisions or scales, on C A, C B, and B A. Therefore:

First.—*To construct or set a triangle, having two of its angles and the side between them given.*

To set a triangle of which 2 angles and the side between them are given

Set the limb C G, to the division at G upon the *arc* answering to one of the angles, say A, and make it fast, then to this ∠A add the other given angle, (which we will call C) and set *the other limb* C H, and make it fast at the division H, on the arc answering to the sum of their degrees; then on the *limb* C G seek the length of the given side C A; next, push the *perpendicular* up or down, till the *parallel* cuts the point A, (always observing, the divided edges are those you work to), and by the help of the mill-headed *nut*, move the *perpendicular*, till its top corner just touches the *limb* C H, say in the point B; when it is evident that the degrees on the arc H I, added to 90°, is equal to the angle B, and that the other sides C B, and B A, may be read off thereon. Or supposing C B D to be the triangle, whose angles B and C and side B C are given, we have only to move the *limbs* so as to make I H equal to B, and H G equal to C, and then to bring the top of the *perpendicular* to touch C H, at the division B, answering to the side C B, when the other ∠ D will be shown by the division on the *arc* G F, adding 90° thereto; and the remaining sides C D and B D may be read off on their respective scales.

Second.—*To set a triangle, having two sides and the angle included between them given.*

Two sides and the angle between them given.

Let A B C be the triangle, A B and A C the given sides, and A the given angle; first set the *limb* C G to the division answering to A, then bring the *parallel* up to the point A, answering

swering to the side C A, and by the *nut* move the *perpendicular*, till B A answers to the given side B A; next bring down the *limb* C H to touch B, and on C B may be read the other side, while H G will show the angle C, and I H + 90° the ∠ B, whence all the six parts are known.

Third.—*To set a triangle, having two sides and an angle opposite to one of them given.*

Two sides and an angle opposite to one of them given.

Let A B C be the triangle, A C and C B the given sides, and A the given angle; first read the angle A on F G, and set the *limb* C G thereto; then push up the *parallel* to the division at A, answering to C A, and with one hand work the *nut* and with the other move the *limb* C H, till they touch at B, the division answering to the side C B; then B A is the side sought, and the arc G H will show the ∠ C, and I H + 90° the ∠ B.

Fourth.—*To set a triangle, having two angles, and a side opposite to one of them given.*

Two angles and a side opposite to one of them given.

Let A B C be the triangle, A and C the given angles, and B A the given side; first, set F G to the ∠ A, and G H to the ∠ C, then push the *perpendicular* up or down with one hand, while the other works the *nut*, till the given side B A, on the *parallel*, is applied exactly between the *limbs* C H, and C G, then I H + 90° will show the remaining angle B, and on C B, and C A, may be read the lengths of those sides.

Fifth.—*To set a triangle whose three sides are given.*

Three sides given.

Let A B C be the triangle; on the *limb* C H seek the point B, answering to the side C B; then, using one hand to move the *perpendicular*, and the other to turn the *nut*, let an assistant at the same time, with his right hand, gently move the *limb* C H, while you cause the top corner of the *perpendicular* always to touch the point B; at the same time let the assistant move the *limb* C G with his left hand, till the lengths of C A, and B A, on their respective scales, are found to intersect each other, when F G will show the ∠ A, G H the ∠ C, and H I + 90° the ∠ B.

The instrument here less convenient

My solution to the last problem is inferior to the common method of plotting the triangle on paper, and measuring the

the angles with a protractor; but I have introduced it here, to show that my instrument is capable of solving this, as well as all other cases of obtuse-angled triangles, and might, by extending the *arc* to a semicircle, as shown by the dotted lines on the figure, solve any triangle. In the practical problems in surveying, which follow, the triangles can always be taken right or obtuse angled, and the instrument as at present constructed is fully competent. I might here add, that a given line can readily by my instrument be divided into any number of equal parts; drawings might be enlarged or diminished, as readily as with the proportional compasses, and many other equally useful purposes may be effected thereby.

than the common method.

Line divided by it into any number of parts.

First.—*To measure an inaccessible distance, by a perpendicular line set off towards the right hand, from the line or base between the observer and objector.*

Set the *base line* of the instrument in a line pointing to the object, at the same time place a staff at any distance at pleasure, as a perpendicular (being 90 degrees from the base). On this perpendicular measure any distance (say 50 yards or other measures) as a second station; move the instrument to this distance, and place it with its *perpendicular* in the same line as before; the instrument being so placed, set the *lower-limb* pointing to the object, and with the screw make the same fast; this done, the distance of the object will be thus readily known. Raise the moving *perpendicular* of the instrument to the division 50 (as before suggested), then with this height move the same by means of the *nut*, till the extremity intersects the *lower limb* before set, at which intersection, the distance from the second station will be shown; and on the *base line* will also at the same time be seen the distance from the first station: this is a case of right-angled triangles.

Method of measuring an inaccessible distance.

Note.—As the divisions on the *perpendicular* are denominated (either feet, yards, poles, or other measures), so will the distances be indicated on the other *limbs*, and on the *base* of the instrument.

Secondly.—*To determine the distances of any two inaccessible objects, both objects lying in a right line from the observer.*

As before directed, place the instrument with its *base* in

To measure the the distance o

two inaccessible objects in a right line.

the line of the objects; then by means of the *upper limb*, set at 90 degrees, place a staff as a perpendicular at any distance at pleasure (say 50 as before). This done, remove the instrument to this second station, and place it so that the *upper limb* (still at 90°) may be in the same line as when at the first station; this done, move the *upper limb* into the direction of the nearest, and the *lower limb* into the direction of the most distant object; which *limbs* being so set, and made fast, the distance of both objects from the second station will be seen on the two *limbs*, and the distance from the first station at the same time seen on the *base line*, by setting and moving the *perpendicular* as directed in the last case. This is also a case of right-angled triangles.

Thirdly.—***To measure an inaccessible distance in an oblique angle, where a right angle cannot be obtained, by reason of some impediment on the ground.***

To measure an inaccessible distance in an oblique angle.

At the first station, from which the distance is required, place the instrument; then set up a staff in any attainable direction, to any distance at pleasure (the more distant the better). The instrument being set with its *base* in direction to the staff, with one of the moving *limbs* take the angle of the object, and with the screw fix it thereto. This done, move the instrument in the direction of its base (being between the first station and staff set up) to any certain distance, (say 50 yards or measures) as a second station. From this second station again take the angle of the object, and thereto fix the other moving limb; this done, the distance both from first and second station, as also the bases and perpendiculars thereto will thus readily be seen. Set the *perpendicular* at random to any height, move the same till the upper point intersect the *upper limb*, or that most distant from the *base*, then read off on the *parallel*, the divisions parallel to the base subtended between the two *hypothenuses* or *limbs*; if this distance or division be equal to the distance measured on the base line, (*i. e.* 50) then the distance of the object from both stations will be shown on the two *limbs*, as will also the base and perpendicular on the respective lines. If the divisions on the *parallel* do not agree with the distance measured, the *perpendicular* must be altered till that division is shown, when

when the required distance will be given. This is a case of our first problem.

Fourthly.—*To level, or measure the altitude of any object.*

To measure the altitude of an object.

It is only necessary to set the plane of the instrument vertical, instead of horizontal, by means of the joint under the instrument, whence it is evident every case may be known as on the horizon; and to level, it is only requisite to set the *spirit level* at the back of the instrument, the *base line* and every object cut by the same will be level thereto.

Fifthly.—*To take angles or altitudes at sea, where the instrument cannot be made stationary.*

To take angles or altitudes at sea.

For this purpose, it is first requisite to change the *sight* a, *Fig.* 1, and substitute the one b, *Fig.* 2; which being firmly fixed and adjusted at right angles with the upper limb, it is evident that when by reflection any object be brought to coincide on the *mirror*, at the extremity of the *base line*, with another object seen in the direction of such *base*, the angle will then be known, being double what the upper limb denotes on the *arch*, to which true angle, or its double, the *lower limb* may be fixed, leaving the one with the *mirror*, again at liberty to take another observation and angle, at any distant place, or time; which being so taken, this *limb* may be also moved and fixed, to double its apparent angle, and the altitude or distance be then determined, by setting the *perpendicular* and *parallel* as in other common cases on land.

Supersedes the use of calculations and tables of sines and tangents.

From this mode of determining distances, as the use of calculations and of tables of sines and tangents are superseded, it is presumed that much convenience will arise to the unlettered who may have occasion to use it, and thereby the errours of calculations will be avoided.

Various other uses to which it is applicable.

As well as the before-mentioned purposes to which the instrument applies, it is presumed there will be found other things which it will perform, some it is hoped useful, and some amusing, amongst which may be enumerated, Multiplication, Division, Rule of Three, Double Rule of Three, &c.; determining the area or sides of any sort of triangle from any proper data; determining the inscribing or inscribed circle

of any triangle, square, or polygon, showing a mean proportional between two numbers, &c.

It is presumed, that an instrument, if perfectly made, on a large scale, would be found very useful and accurate in various practical calculations, as well for making them, as for proving them after made in figures.

The following are specimens of the manner of calculating by this instrument.

First Question.—*If £100 in 12 months produce 80 shillings interest, what will £200 produce in 18 months, and also what will it produce in 12 months?*

Question in interest worked by it.

On the *base line* of the instrument set £100. On the *perpendicular* set 80 for shillings interest. Then bring the *lower limb* to intersect, which angle will then be, as per question, equal to 12 months at all places on the *base;* having so fixed the *lower limb*, move forward the *perpendicular* till it intersects the *lower limb* at the height 12 on the *perpendicular*, then raise the *perpendicular* to 18, and to the extremity thereof fix the *upper limb* to intersect, which angle will then be in proportion as 18 to 12 to the *lower limb*, being equal to the different times. The limbs being so fixed, it is only requisite to move the *perpendicular* to 200 on the *base;* and, raising the *perpendicular* till it intersects the upper limb, you will have thereon the answer 240 shillings, and at the same time, at the intersection on the lower limb, 160, being the interest for 12 months only.

Question second.—*To determine the inscribed or inscribing circle of any polygon, the side being given; for example, a hexagon the side of which is 100 feet.*

To determine the inscribed or inscribing circle of a polygon.

Set one of the limbs to half the angle included in the required side of the hexagon (*i. e.* 30 degrees), then set the *perpendicular* to the height of half the side given, being as per question 50. Then move the *perpendicular* till the extremity intersects the *limb* before set, on which, at such intersection, will be denoted the radius of the inscribing circle, and at the same time may be seen on the *base* the radius of the inscribed circle.

Questio

Question third.—*To find a mean proportional as between* 600 *and* 200.

This depends on the well known property of a right angled triangle.

Set the *perpendicular* on the *base* line, at the distance of half of the difference of the two numbers (*i. e.* $\frac{600-200}{2}$); this done, raise the *perpendicular*, and move either of the *limbs* till the extremity of the *perpendicular* intersects thereon at half the sum of the numbers, being 400. This done, the height of the *perpendicular* will show the proportional required, being 347. To find a mean proportional.

Note.—On the plate on which the perpendicular slides will be found Noniuses for subdividing the divisions on the base or perpendicular into 10 divisions.

Reference to the Engraving of Mr. Salmon's Geometrical Quadrant and Staff, Plate V.

Fig. I represents the face of the quadrant, on which A is the fixed base line; B the moveable perpendicular; C the upper limb; D the lower limb; E the arc; F the nut, which moves the perpendicular by means of a rack and pinion. Explanation of the plate.

G, a spring to keep the perpendicular steady; H, a screw for fixing the joint of the staff; *a* the eye piece, or sight, with a small hole in its centre; I, I, I, the sights for direct vision, consisting of only a small slit in each. When objects are to be viewed by reflection, as with a Hadley's quadrant; the sight *a* at the centre is taken off, and the sight *b* with a mirror, shown at *Fig.* II on rather a larger scale than the former, must be substituted.

Fig. III.—K is the staff, the mode of applying which to support the instrument when in use is shown by the same letters in the other figures; L is the screw, by which the staff is fixed firmly in the ground.

Fig. IV represents the back of the quadrant; M M are the screws, by which the upper and lower limbs are fastened after taking an observation.

O, a spirit level; H the tightening screw for the joint before noticed; P the socket attached by its joint and tighten-

ing screw to the back of the quadrant; the staff K is screwed into the above socket.

Fig. V. shows the practical method of using the instrument for determining the distances of the objects Q and R from the two stations S and T, at which the instrument is to be successively placed, and used as before described.

Fig. VI. is the diagram referred to at page 204.

Fig. VII. represents the mode of applying the tightening screw H, in *Fig.* I. and IV. by means of the semicircular spring, enclosing the cylindrical stem, or neck of the joint.

X.

Description of a Larum applicable to any Pocket Watch, by Mr. JOHN PRIOR, *of Newfield, near Skipton, in Craven*[*].

Applicable to any watch without injuring it.

IN constructing this machine, I have endeavoured to make it as simple as I could; so that by the assistance of a pocket watch, of any size, or any number of turns of the fusee, which ever way it is wound up, it will cause the larum to let go at any time required, without hurting the going of the watch.

New method of winding up the main-spring,

In winding up the main-spring of this machine, I have used a method different from that of any other person, and which admits of its acting with considerably more power than where thicker pivots are used.

and stopping it without a fusee.

The discovery of stopping the main-spring when wound up without a fusee, I must beg leave to say, gives me a great deal of satisfaction; and will, I hope, be useful to my brother workmen.

To show with what ease a watch will turn the larum screw, I made the following experiment.

Ease with which it acts.

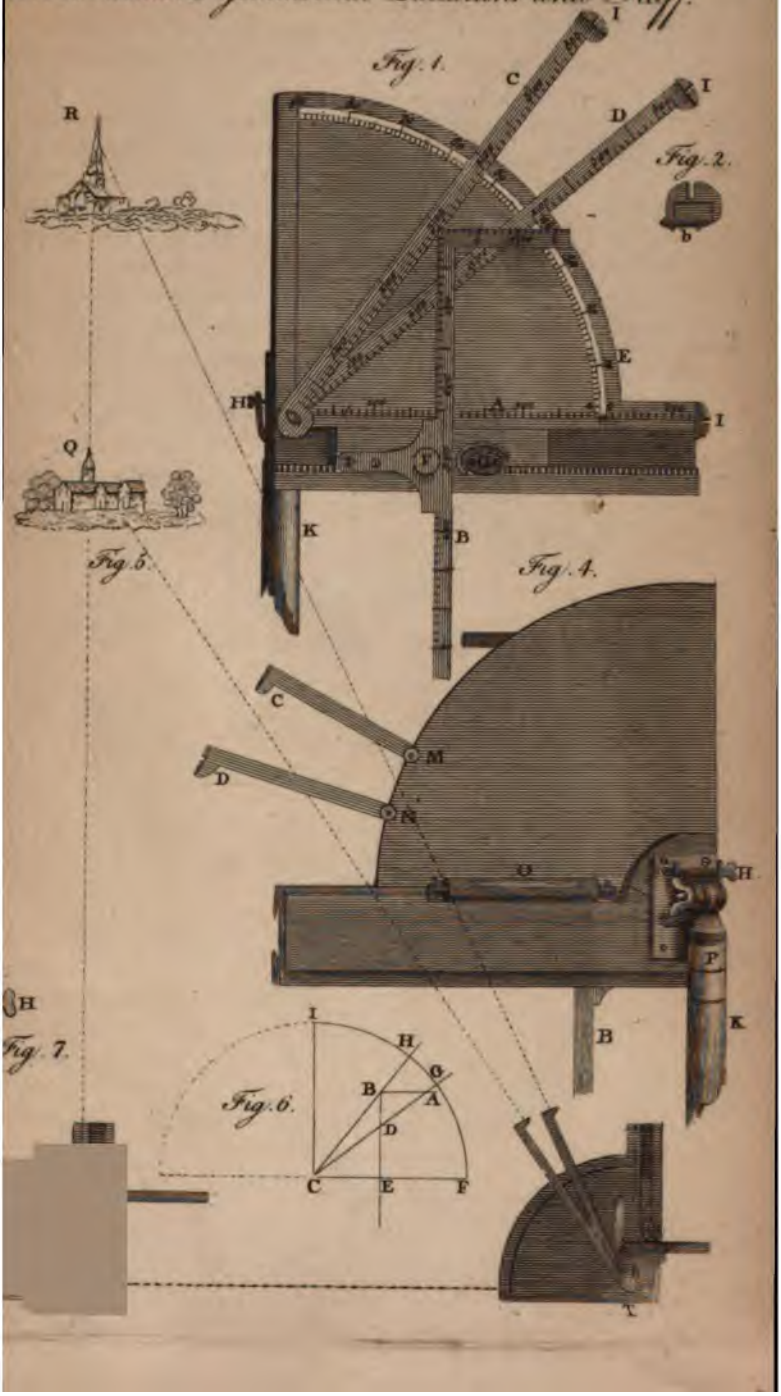
When the machine was wound up and the level put upon the screw, I turned the axis of the screw while one of the pins which has a communication with the key when the

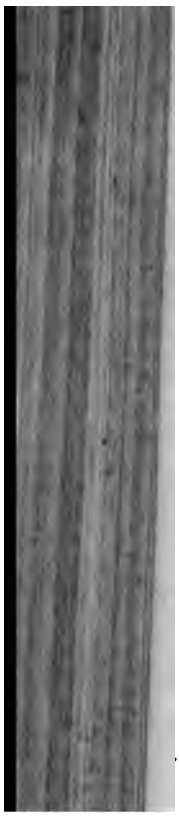
[*] The Society of Arts voted Mr. Prior their silver medal and twenty guineas for this invention. Trans. Vol. XXIII, p. 393.

watch

Nicholson's Philos. Journal Vol. XIII. Pl.

Mr. R. Salmon's Geometrical Quadrant and Staff.





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watch is put to the larum, was parallel to the horizon; its distance from the centre of motion was one-eighth and three-fourths of an inch. I hung a slender thread upon the pin, with ten grains troy weight tied to it, which moved the screw.

This larum may be set a week before-hand, if the watch would go as long, by increasing the number of threads of the screw. May be set as long before hand as the watch will go.

When a watch is made to wind up the contrary way, it is necessary only to take out the pin in the axis of the detent, and turn the lever the other side up, and then it will drop off at the other end of the screw to disengage the larum. Left hand watch.

By taking notice at what hour you wind up your watch, and by winding it up again at twenty-four hours, you will ascertain how many hours are contained in one turn of the fusee, the most common are four, five, six, hours for each turn. I have divided the common index into 120, which does for three concentric circles. That next the centre is for a watch fusee of four hours a turn; the next five, and the last six. While the hand of the larum passes over one of the divisions, it will be equal to two minutes to the four hours circle, two and a half to the five hours, and three minutes to the six hours circle, all shown by the same hand. Number of hours in a turn of the fusee.

Before a watch is put to the larum, it must be fitted with a key that will not drop off when the watch is turned with the key-hole downward, something like that which I have sent; then hang up the watch by the pendant to the holder, which may be turned while the key end on the watch is opposite to the axis of the screw, and the face of the watch parallel to the plate. Then turn the sliding pieces any way, as may suit the watch in that situation, and screw it fast. A particular key necessary.

Supposing the watch is four hours in making a turn of the fusee, turn back the hand of the larum while one of the pins touches the pin in the watch key, and if the hand is not at top, turn it back till it is. If the larum is required to go off in four hours, lift the lever into the first turn of the screw; if in five hours, turn back the larum hand one hour; if in six hours, two back; if in seven hours, three back; and if in eight Method of setting it.

eight hours, do not turn it back, but put the lever into the second turn of the screw; and so for the rest.

I am, Sir,

Your very humble servant,

JOHN PRIOR.

Reference to the Engraving of Mr. Prior's Larum, Pl. VI.

Explanation of the plate

Fig. 1 shows a bird's-eye view of the machine.

A, shows the position of the watch on the larum.

B. The spiral cylinder, fixed on the axis, and moved by a pin across a key placed on the fusee square of the watch.

C. The acting lever, one end of which lies upon the spiral, the other end is movable upon an arbor D.

E, shows a notch cut in the arbor D. This notch is cut more than half through the arbor, in a situation opposite to a pin at F, in the middle of the rim of the larum contrate wheel G. When the lever C falls off the cylindrical spire B, the notch E is moved to a situation so as to allow the pin F to pass through the notch which discharges the larum hammer H, which works by pallets in the contrate wheel G in the usual manner.

O. The barrel which contains the spring, the inner hand of which is connected with the same axis as the contrate wheel.

K. The finger piece which winds up the spring.

b, b, b. The three sliding pieces which hold the watch.

c, c. Two projecting pins, which are carried round by the pin *d*, which is fixed across the key fitted to the fusee.

Fig. 2 shows that side of the machine on which the watch is fixed.

a. The sliding piece, on which the pendant of the watch is hung.

b, b, b. The three sliding pieces which serve to adjust the watch, and hold it in such a position that the fusee square may be in a line with the axis of the spiral cylinder B, shown in *Fig.* 1.

I is a ratchet wheel, on the centre of which is a button or finger-piece K, to wind up the larum spring.

L. The

Nicholson's Philos. Journal Vol. XVIII. Pl. VI. p. 1

Mr. John Prior's Larum for Pocket Watches.

Fig. 5.



Fig. 3.

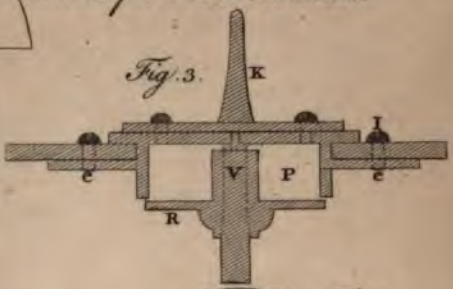
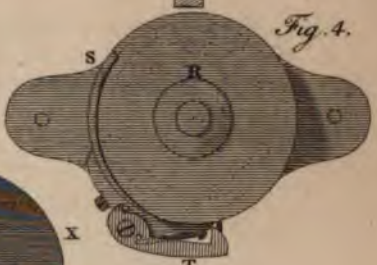


Fig. 4.





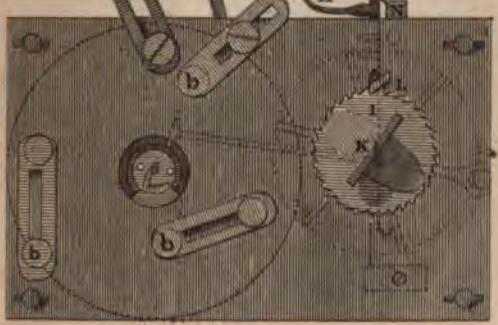


Fig. 2.

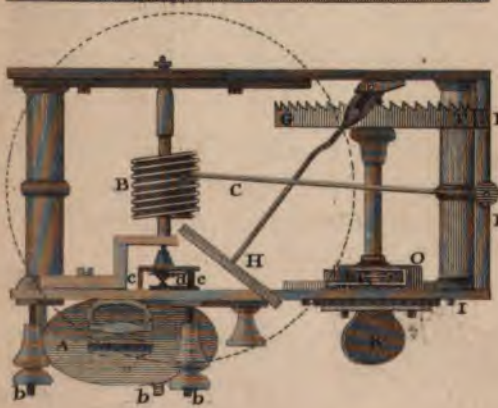
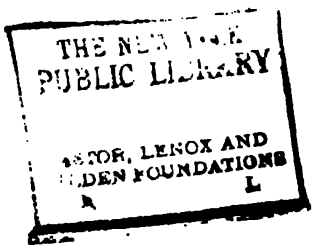


Fig. 1.


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L. The click which works in the teeth of the ratchet wheel.

H. The lower part of the arm of the hammer.

M. The bell, within which the hammer strikes.

N. The cock in which the pivot of the axis of the pallets acts, and to which the hammer is connected.

Fig. 3, shows a section of the ratchet wheel and spring barrel, which are screwed together, and move at the same time, but are kept in their place by two pieces *e*, *e*, which fit a groove in the barrel.

K. The button or finger piece.

I. The ratchet wheel.

P. The box for the spring.

R. The cap which covers the spring box.

V. The axis on which the main spring is wound.

Fig. 4. R. The cap, under which is the spring. On the edge of this cap, at S, is an indent to retain the spring when wound up.

T. A lever, with a hook at its end.

At the end of this lever, on a line with the hook, is a small piece of steel, which goes through a hole in the box, and presses upon the main spring; so that, when the main spring is wound up on the axis V, the hook is at liberty to fall into the way of the indent S, and is there stopped.

Under the lever T is a small spring, which presses it against the main spring within the barrel.

Fig. 5, shows the index of the larum; the outside circle of which is divided into 120 parts, which index serves for three concentric circles; that next the centre is for a watch fusee of four hours a turn, the next five, and the last six, as explained in the letter.

XI.

Observations on the Combination of fixed Oils with the Oxides of Lead, and with Alkalis: by Mr. F. FREMY, *Apothecary, of Versailles**.

SCHEELE was the first who observed, that the water, which serves as an intermedium when fat oils are combined with

Water used in making litharge plaster is sweet.

* Annales de Chimie, Vol. LXII, p. 25, April, 1807.

with litharge, hold in solution a substance, that he called the sweet principle of oils, because it has a very decided saccharine taste. But as, according to the observations of that eminent chemist, the water likewise holds in solution a certain quantity of oxide of lead, may it not be inferred, that this taste is owing to the property that metal has of imparting sweetness to most of its compounds? If experience prove the contrary, would it not be interesting, to inquire how this principle is formed? what are its properties? in what state the oil is left, after having lost the principles that give birth to it? whether this abstraction be indispensable to the combination of oil with oxide of lead? and on the experiments necessary in this research to establish the theory of one of the most important operations of pharmacy and the analogy of its results to alkaline soaps?

Sweet principle of oils.

Is it not owing to the lead?

Questions to be answered.

Such were the propositions that led to the experiments I am about to describe.

Into a tubulated glass body I put equal parts of olive oil, litharge, and water. To its tubulure I adapted a tube, terminating in a vessel of lime water; and to its orifice a bladder, to prevent the contact of air. This bladder was so contrived, as to allow me to stir the matter with a spatula, so as to prevent it sticking to the bottom. Having brought the mixture to boil, I observed the oxide of lead change in succession from red to yellow, and from yellow to white: and during the experiment carbonic acid was almost always flying off. Having suffered the apparatus to cool, I examined the results of the experiment in succession.

Oil, litharge, and water boiled together.

The oxide changed yellow, and others white,

and carbonic acid evolved.

The water, that had served as an intermedium, had a strong metallic taste. With the addition of yeast, and at a proper temperature, I could never bring it to ferment*. It formed an evident precipitate with sulphuric acid, and with hidroguretted sulphurets†. I passed sulphuretted hidrogen through it, till nothing more was thrown down, and then filtered to separate the sulphuret of lead.

The water had a metallic taste, & would not ferment.

Lead precipitated from it

* I was for a moment led into an errour, by employing yeast, which, not having been washed, contained some alcohol.

† I satisfied myself by various experiments, that it is of no consequence to the solution of oxide of lead, that the oil or fat should be rancid, as Scheele supposed.

Oil dissolves lead without being rancid.

The

The filtered liquor still retained a strong saccharine taste. It was evaporated to the consistence of a sirup, and the acetate of lead then no longer indicated the presence of sulphuretted hidrogen. My attempts to ferment it were as unsuccessful, as before the oxide of lead was separated. Exposed to the air, it strongly attracted moisture: thrown on burning coals, it flamed like an oil: on boiling it with the red, yellow, and white oxides of lead, it dissolved only the yellow: on distilling it repeatedly with nitric acid, oxalic acid was formed: distilled in a retort on an open fire, part of it rose, as Scheele observed; and by increasing the heat the products were an empyreumatic oil, acetic acid, carbonic acid, carburetted hidrogen gas, and a light, spongy coal, containing no oxide of lead.

The filtered liquor sweet. Evaporated. Would not ferment. Attracted moisture; burned with flame: dissolved yellow oxide of lead: formed oxalic acid. Its products.

From what I have mentioned it might be presumed, that the oil, when it had combined with the white oxide of lead, was not in the same state as before the combination.

To free it from this oxide I employed acetic acid, because the solubility of acetate of lead would afford ready means of separating it from the oil, the properties of which I wished to examine.

The oil separated from the oxide.

This oil has the consistence of soft fat, and the taste of this animal substance when rancid. It is insoluble in water, but soluble in alcohol*; from which it is precipitated by water as volatile oils are, like them partly rises in distillation.

Its characters.

The slightest boiling is sufficient to combine it perfectly with white oxide of lead, and give it the consistence of a strong plaster, which does not take place with litharge, or with massicot.

Readily unites with white oxide of lead.

The yellow and white oxides of lead cannot combine with common oils. I satisfied myself of this fact by boiling them together much longer, than would have been necessary if I had used litharge.

Neither white nor yellow oxide unites with common oil.

From these experiments it follows, that, when fat oils are treated with litharge, the oxigen of the latter takes from them carbon, and previously hidrogen, to form with them water and carbonic acid.

Oxigen of litharge takes carbon and hidrogen from oils,

* All fat oils are soluble in alcohol; but they are far from possessing this property in so striking a manner, as after they have been boiled with litharge.

That

and thus produces the sweet principle.

That this abstraction, rendering the oxigen more abundant in the oil, gives birth to that saccharine substance, which Scheele calls the sweet volatile principle of oils.

Differs from mucilage and sugar.

That this sweet principle differs from the mucoso-saccharine by its property of dissolving the yellow oxide of lead: that its presence is independant of the presence of oxide; and that it differs from sugar by its volatility, and by the impossibility of bringing it to ferment.

The oil acquires some properties of volatile oils.

That the oil, deprived of the elements that give birth to the sweet principle, and the quantity of hidrogen and carbon that constituted it fixed oil, acquires several of the properties of volatile oil.

The only state in which it combines with lead.

And finally, that this last state of oil is the only one, in which it can combine with white oxide of lead.

Are plasters metallic soaps?

From the knowledge I had thus acquired of the theory of this combination of oils, I thought I should not neglect to examine how far the opinion of several chemists, who consider plasters as real metallic soaps, is well founded. The analogy between plasters and soaps can be confirmed only by observing in their respective combinations a similarity of phenomena, or at least of results.

Strong lie mixed with oil.

I mixed very pure soapboilers lie with olive oil, and exposed the mixture to the air under a glass jar. A week after there was but a very slight absorption: the soap had still a strong alkaline taste; and the oil of this soap did not dissolve entirely in alcohol. But at the end of six weeks the absorption of oxigen was complete; the soap was very white, and of a good consistence; the taste of alkali in it was faintly perceptible; dilute sulphuric acid extricated from it carbonic acid; and the oil proceeding from this decomposition had the same consistence as that from plasters, dissolved very readily in alcohol without the assistance of heat, and was precipitated from it by water.

No sweet principle in water after making soap.

I made some soap in the same way as soapboilers, and very attentively examined the liquor, that remained after the soap was completely formed; but I could not discover in it any trace of the sweet principle.

The effect different in degree, rather than in kind.

As the absence of this principle in making an alkaline soap probably depends only on a greater or less abstraction of carbon or of hidrogen; and in other respects the action of

of oxigen on the oil, and the state of the oil, are absolutely the same in the fabrication of plasters and of soaps; I conceive, that plasters ought to be considered with respect to soaps the same as insoluble metallic salts are to alkaline salts.

I satisfied myself, that the want of consistency in soaps made with potash is not at all owing to the state of the oil, but to the nature of the compound; for by treating with potash the oil obtained from a very hard soap of soda, I could form nothing but a soft soap. Soaps with potash, soft from the nature of the alkali.

XII.

Account of a pretended pure native Magnesia.*

THE German dealers in minerals sell under the name of pure indurated magnesia, and as coming from Moravia, a mineral substance found in amorphous masses of the size of the fist, covered with an earthy crust of a yellowish white colour, that slightly adheres to the tongue, and is a little greasy to the touch. Its fracture is dull, entirely compact, imperfectly conchoidal, and approaching to a plane: its colour is yellowish white: it has small cavities, lined with little mamillary projections, that appear to be composed of crystalline points when viewed in a strong light: it is sufficiently hard to scratch glass, and steel leaves a coloured mark on it, but it does not strike fire with steel: its specific gravity is 2·83: at its edges it is very transparent: it does not absorb water, or adhere to the tongue. Fossil sold as pure magnesia by the German dealers. Its characters.

Before the blowpipe it cracks, and does not melt: it is not at all phosphorescent: it dissolves with effervescence in the nitric and muriatic acids; with the sulphuric it occasions a copious precipitate.

It cannot be confounded with the native magnesia of Werner, which is easily scratched by steel, strongly adheres to the tongue, and is of the specific gravity of 2·88. Difference of native magnesia.

This mineral, analysed by Mr. Bucholz, yielded in 100 Its component

* Journal des Mines, January, 1807, p. 75.

parts

parts. parts 28 of pure lime, 20·5 of pure magnesia, 1·5 of manganese with a little iron, and 48 of carbonic acid.

A muricalcite. Hence it appears, that it is a muricalcite, or a variety of the bitterspath of the Germans.

XIII.

Some remarkable Occurrences in Natural History. From the Rev. James Hall's Travels in Scotland.

WHILE Mr. Hall was at Elchies, on the north-west banks of the Spey, about fifteen miles from Elgin, he saw a remarkable migration of eels. The following are his own words.

Migration of eels. When I first observed them, it was about one in the afternoon of a Sunday. How long the eels had been transmigrating before I know not. They continued making their way up the river all that day till about eight in the evening, when it grew dark. They began again early next morning, but how long before five I cannot say. They continued to migrate for three whole days after I observed them, with only an interval of a few hours in the night. They kept as near the north-west edge of the river as they could: and, when there were bays the edge of it, they went regularly round these, whether great or small. They were about ten abreast, and each eel about three and one half inches long: they marched at regular distances, which might be about four, or rather three and one half feet. There were stronger eels as a guard, and generally about five or six inches long. I observed the smallest and weakest ones always kept nearest the edge, where the current was least. From an accurate calculation a hundred passed every minute, making six thousand per hour.

In lines abreast at regular distances.

Smallest next the shore.

From a little before sunrise to a little after sunset. They proceeded at this rate for three days, from about half an hour before the sun rose till about half after he set, making about sixteen hours each day, in all about forty-eight hours, which, multiplied by the six thousand that passed every hour, make two hundred and eighty-eight thousand, most of which I saw pass; but whence they came, or what they were in quest of, I know not. They did not stay for one another, but each made the best of its way, wriggling

wriggling with the utmost celerity; and when I pushed any of them farther into the river, they always came to the edge as fast as they could. Not one but had its head up the water. They seemed to be in great haste, and breathing hard, as small bubbles of air often rose up to the surface; and when, having caught any of them, I turned its head downward, so as to swim with the current, it would not, but with all the expedition in its power joined its new companions, and wriggled on along with them. As I could not be always there, I appointed others to watch their motions, and I found, though I could not see exactly how they acted, that, during the time it was beginning to grow dark, by a kind of signal, they all at once hid themselves in the sand or mud for miles at the same instant, and seemed not only under the command, but the protection of the larger ones, that, like officers, commanded them. Indeed, I saw sometimes large eels from twelve to fifteen inches long, making up the water now and then, about three or four yards farther towards the middle of the river, and about five and twenty yards behind one another; but whether they were connected with the general emigration I know not, though I rather suppose they were, as they were never above twelve or thirteen feet from the small eels, and often seemed to turn an anxious look towards their young friends. The young ones, as they were near the edge, were seldom an inch below the surface. Those about five or six inches long might be between one and two inches below the surface, being in deeper water, and the large eels went at a much greater velocity than the small ones. But, if they had any connection, or care of the small fry, they must sometimes have stopt short, or slackened their pace. I have seen the horse and foot guards reviewed by his majesty, &c. &c. in Hyde Park, and ten thousand men performing the same action at the same instant of time; but the eels in the river Spey kept their ranks as regularly, and seemed to be as subservient to the greater ones, as any of the corps at a review are to the command of their officers.

Travelled hastily.

All at once hide themselves in the mud towards night.

Apparently under the command of large ones.

Another fact he observed may be mentioned as an instance of the resources of animals, when prevented by circumstances from following the usual practices of their species.

Necessity

grains. The residuum was still very black, pasty, and adhered to the iron with which it was stirred. On washing it was reduced to two drachms 8 grains.

to about one. These two drachms 8 grains were calcined another hour, which reduced the weight to about 60 grains; and were then

Gave 18 grs. of saline extract. lixiviated. The lixiviums mixed together yielded 18 grains of a saline extract, the taste of which was not perceptibly alkaline.

Phosphate of potash. These 18 grains, redissolved and dried, would not crystallize, and were reduced to 14. Suspecting that the potash was saturated with phosphoric acid, I dissolved it in distilled vinegar, and afterward treated it with alcohol. This operation reduced it to 11 grains of that acidulous phosphate, which crystallizes in tetraedral prisms terminated by similar pyramids. I forget whether the faces of the pyramids answered to the faces or edges of the prism.

If 10 drachms of charcoal, the produce of 41 of maize, gave 14 grains of phosphate, 100 drachms of maize would yield but 34 or 35 grains, which is far from 40 per cent, as mentioned in Delamétherie's Journal. So great a difference could not have escaped such a man as de Saussure: it must have been an errour of the press therefore, or of the manuscript.

The obstinacy with which the charcoal of maize resists burning is astonishing: animal charcoal could not exhibit more.

Charcoal from the stalk burns in about half the time. The same plant affords a charcoal of very different kind. The charcoal of the stalk, triturated with five sixths its weight of saltpetre, is consumed in a tube of a given diameter in 28 seconds. A similar mixture made with the charcoal of the grains requires 52 seconds for its being consumed in the same tube.

To Correspondents.

The communication with which N. R. D. promises to favour me will be very acceptable. I have likewise to thank him for his concluding hint, and shall certainly avail myself of the source it points out.

Mr. Cayley's communication is received, but on account of the engraving it could not be inserted in the present month.

I hope to be able to give some correct Observations and Results in our next respecting the Comet, which is at present visible.



Structure of covered ways.

Fig. 1.

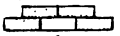


Fig. 2.

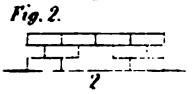


Fig. 3.

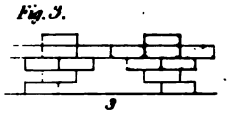


Fig. 4.

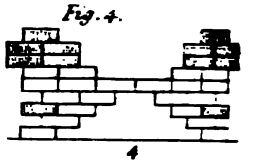


Fig. 5.

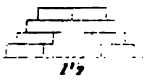


Fig. 6.

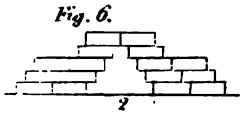


Fig. 7.

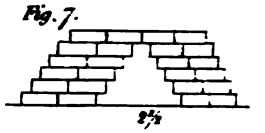


Fig. 8.

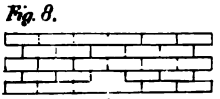


Fig. 9.

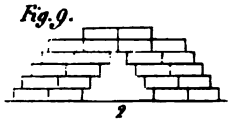


Fig. 10.



Fig. 11.

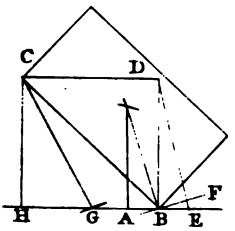


Fig. 12.

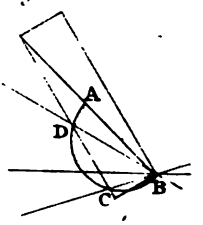


Fig. 13.

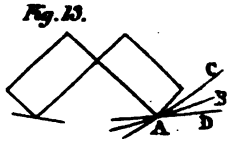


Fig. 16.

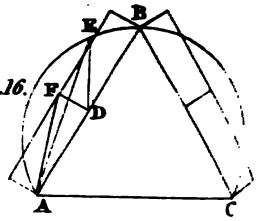


Fig. 14.

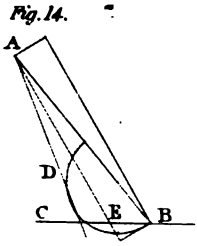


Fig. 15.

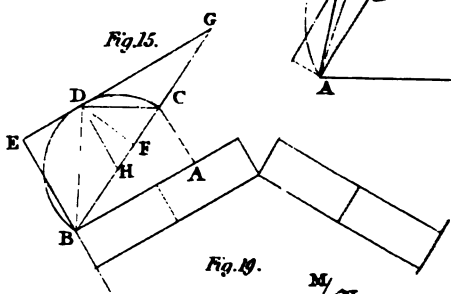


Fig. 17.

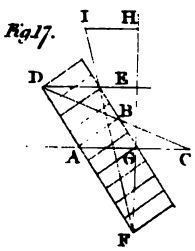


Fig. 18.

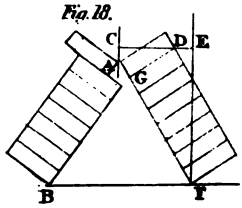


Fig. 19.

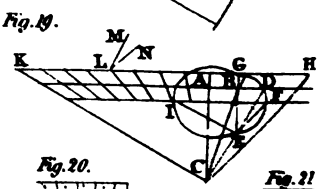


Fig. 20.



Fig. 21



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1807.

ARTICLE I.

Remarks on the Structure of covered Ways, independent of the Principle of the Arch in Equilibrium, and on the best Forms for Arches in Buildings. From a Correspondent (APSOPHUS).

To Mr. NICHOLSON.

SIR,

THE subterraneous passages or tunnels of the Babylonians, and perhaps the cloacae of the Romans, were constructed, according to the opinion of the best informed antiquaries, by simply causing the bricks or stones of each of the side walls to project more and more as they rose higher, till they finally met in the summit. The most ancient remains of the Grecian buildings, for example, the treasury of Atreus at Mycenae, and other ruins in the Peloponnesus, exhibit in general over their doors, according to the reports of modern travellers, a triangular aperture, formed by large stones; the base of the triangle coinciding with the lintel of the door; and the pointed arches of the Gothic buildings are by no means universally so arranged, as to derive their stability from the proportion of their curvature in every part, to the pressure which would be produced, according to the commonly received theory, by the

Ancient substitute for the Arch.

height of the superincumbent wall. As far as I know, this subject has not been mathematically investigated in all its parts, and I shall therefore submit to the consideration of your readers some propositions relating to the stability of overhanging walls and of triangular covered ways.

Bricks overhanging till they meet in a point.

I shall examine those cases only, in which the materials employed are equal rectangular parallelopipeds, whether bricks or wrought stones, and in the first place I shall suppose them destitute of all friction or adhesion, and placed horizontally. With such materials, it may be shown from the principles of the lever only, that a covered way may easily be made, not exceeding in breadth the length of three or four bricks or stones, and that the combinations, represented in Pl. VII, fig. 1··7, will stand in equilibrium without external support: and that if the breadth of the way be equal only to the length of two bricks, it may have any height of wall added over it without destroying the equilibrium (Fig. 8). These combinations are however incapable of resisting the pressure of any considerable force, and the method of building in this manner cannot be generally advisable; but the weight of two bricks is supported at the vertex in Fig. 9, and by extending the basis, and heightening the wall at the sides, a much greater strength might be obtained. It is however obvious, that a wall terminated in this manner would by no means necessarily exert such a pressure on any stones, forming a facing of the oblique surface, as is commonly supposed in the theory of the arch; on the contrary it is plain, that an arch might be turned under it, which would be sufficiently strong for every purpose, if capable of supporting little more than its own weight: and the same reasoning is applicable to the wall in contact with the lower parts of every common arch. Hence it becomes often eligible to construct the arch in such a manner as to be more capable of resisting a pressure near its vertex; and thus its form will approach in some degree to that of a pointed arch. The arches of bridges, on the contrary, have to support the pressure of materials of a very different description; and for this reason their greatest curvature should be near the abutments.

To support much weight the width must be small.

A stronger form.

Arch turned under it.

Pointed arch.

Arches of a bridge.

Requisites to

In the next place I shall inquire into the conditions requisite

site for the stability of an oblique facing, composed of rectangular bricks or stones only, both with and without the consideration of the effects of friction. The simplest case that can be proposed is that of two bricks meeting each other, and standing on a perfectly smooth and horizontal plane, the centre of gravity of each being vertically above the lowest angle (Fig. 10). But if the base be widened, the surfaces supporting the bricks must be rendered oblique. The weight of the brick acts on a lever of which the length is A B (Fig. 11), in turning it round the point B; and this is resisted by the horizontal thrust at C acting on the lever B D, hence the horizontal thrust must be to the weight as A B to B D, and making B E = A B, the horizontal thrust at B combined with the weight will act in the direction D E, and the brick will be supported by a surface B F perpendicular to D E. Supposing the thickness of the brick inconsiderable, the centre of gravity being in the line B C, taking B G half B H, the line C G will be perpendicular to the surface on which it will rest in equilibrium; and this theorem may be of considerable use in carpentry, for finding the best possible direction for the abutment of a rafter. If the abutment is in the direction of the end of the block Fig. 12, describe on half the diagonal, A B, the semicircle B C D A; and C B, or D B, will show the position of a line, which being made horizontal, the block will be supported in equilibrium. If the horizontal line cross the circle between C and D the end B will slide downwards, but if between A and D, or B and C, it would be urged upwards, but the bearing will be transferred to the lower corner, and the whole will remain at rest: and this will be the case in all positions, when the circle falls wholly within the side of the block, that is, when its thickness is not much less than half its length. Thus two common bricks would remain firm in all elevations if placed with the narrow sides of their ends lowermost; even without any friction: but with the wider sides lowermost, they would slide down the abutments if the distance of their ends were more than about two, and less than fourteen inches.

the stability of an oblique facing.

The same principles applicable to the abutment of a rafter.

Bricks in an inclined position firm when the narrow side is downward.

The last additional circumstance which requires to be examined, with regard to the stability of bricks or stones in

Effects of friction.

oblique situations, is the effect of friction or adhesion. This force may be considered, in all practical investigations, as proportional to the mutual pressure of the surfaces concerned; and the most convenient way of estimating its magnitude is to incline the surfaces to the horizon, until they begin to slide on each other. The angle at which this happens will be always very nearly if not exactly the same for surfaces of the same kind, and it may with propriety be called the angle of repose: and it is obvious, that any other force acting on the surface in the same angle as that in which the force of gravity acts in this instance, will be completely obviated by the resistance of the surface: and the friction will be to the pressure as the tangent of the angle of repose to the radius. If therefore the surface A B (Fig. 13) is calculated to resist the pressure of the block A without friction, by making the angles B A C and B A D each equal to the angle of repose, we may determine the greatest and least inclination which will be sufficient for retaining the block by the assistance of the friction or adhesion; the stability being greatest of all in the original situation A B. In the same manner the rectangular block A B, (Fig. 14) will be supported by its abutment as long as the horizontal line B C crosses the semicircle within the line A D, D A E being equal to the angle of repose.

Convenient mode of measuring it.

Angle of repose.

When two blocks of equal dimensions form an overhanging facing on each side of a triangular aperture, (Fig. 15) the upper one is in the same predicament as if it rested simply on a fixed abutment; the lower one is retained in its situation by the force of friction only. If A B C be the angle of repose, the direction of the force supporting each of the upper blocks will be B C; and if the vertical line B D represent the weight of the block A, B C will be the resisting force, and A C the friction, which counteracts the tendency of the block B to descend along the abutment, and this force is represented by E B. In order therefore to find the postion in which the block B will most readily slide away, we must make the proportion of E B to A C a maximum; and this will happen when the mean of the angles D B A and D B C is equal to half a right angle. For the

Case of a facing of two blocks on each side.

Worst possible position.

sine

sine of the angle D B C being $\frac{DC}{BC}$, and its cosine $\frac{BD}{BC}$, and the sine and cosine of A B C being $\frac{AC}{BC}$, and $\frac{AB}{B}$, the sine of D B A is $\frac{DC.AB}{BCq} + \frac{BD.AC}{BCq}$, and consequently E B $= BD.\frac{DC.AB+BD.AC}{BCq}$, which, divided by A C, is $BD.\frac{DC.AB}{AC.BCq} + \frac{BDq}{BCq}$, and this must be a maximum, consequently, B C being supposed constant, $\frac{AB}{AC}.\frac{BDC}{BC} + \frac{BDq}{BC}$ must also be a maximum. Then if we make D F perpendicular to B C, and the angle F D G = A C B, D F will be $\frac{BDC}{BC}$, F G $= \frac{AB}{AC}.\frac{BDC}{BC}$, and B F $= \frac{BDq}{BC}$, so that B G must be a maximum, which will evidently happen when D G is a tangent to the semicircle B D G, and the angle D B C half D H C, which is the difference between A B C and a right angle. If we wish to determine the proportion of the friction to the pressure when the friction is barely capable of retaining the block in its situation in the most unfavourable position, let x be the sine, and y the cosine of half the angle A B C, then the sine and cosine of half a right angle being $\sqrt{\frac{1}{2}}$, the sine of A B D or B D E, as well as that of B C D, will be $\sqrt{\frac{1}{2}}x + \sqrt{\frac{1}{2}}y$. Now, if the weight be B D, B C $= \frac{BD}{\sqrt{\frac{1}{2}}(x+y)}$, and the sine of A B C being $2xy$, A C is $\frac{2xy.BD}{\sqrt{\frac{1}{2}}(x+y)}$: but the weight which produces the friction is three times the weight of a single block, the friction on the upper surface being derived from the pressure of the highest block, and that on the lower from the pressure of both blocks; while the tendency to descend belongs to the lower block only, and is therefore expressed by B D. $\sqrt{\frac{1}{2}}(x+y)$; hence we have the equation $\sqrt{\frac{1}{2}}(x+y) = \frac{6xy}{\sqrt{\frac{1}{2}}(x+y)}$; therefore $\frac{1}{2}(x+y)^2 = 6xy$, $(x+y)^2 = 12xy$, $x^2 + y^2 = 10xy = 1$, $2xy = \frac{1}{5}$, which is the sine of A B C, and the friction is in this case to the oblique resistance as 1 to 5, and to the pressure nearly as 10 to 47: so that whenever the friction is greater than this, which

Case of the friction being barely sufficient to retain the block.

The friction sufficient to t tain two pair

of blocks in most positions.

which is almost always the case with the materials commonly employed, two pairs of equal blocks meeting each other in this manner will be secure from sliding in every possible position.

When more.

If there are more than two blocks on each side, or if the lower blocks are larger than the upper one, the force tending to support the lower ones, which is derived from the pressure of the upper one, is twice the immediate friction occasioned by its weight, since the same pressure acts in two different places, and as long as this exceeds the difference between the friction and the relative weight of the lower block or blocks, they will be secure from sliding along the abutments.

Friction of common bricks half the pressure.

For example, in the case of common bricks or stone, the friction is at least half of the pressure; for if a brick be placed with the short side of its end downwards on another which is gradually raised, it will fall over before it slides; we may therefore safely estimate the friction as equal to half the pressure, the tangent of the angle A B C being ·5, its sine ·446, and its cosine ·892. Now if the whole aperture be supposed equilateral, the sine of D B A will be ·5, and its cosine ·866; and the sine of D B C nearly ·06; and the friction A C will be to the weight B D as ·45 to 1, and to E B as 9 to 10, so that 18 bricks on each side might be secured from sliding by the double effect of the upper pair.

18 bricks might stand at an angle of 60°.

Two modes in which they might give way.

There are however two other ways in which such a structure might give way: the lower portion revolving on its lowest point, and the higher either moving with it towards the opposite side, or sliding upwards in a contrary direction: and in order that the pile may stand, it is obvious that it must possess sufficient stability in both these respects. When there are only two equal blocks on each side, it is easy to determine whether or no their breadth is sufficient to prevent their both falling inwards, by describing round the triangle A B C (Fig. 16). a segment of a circle, making D E vertical, and joining A E, which must either coincide with the diagonal A F, or be below it. If there are more than two pieces on each side, in order to determine the stability of any joint A B (Fig. 17), let A C and D E be horizontal, and F E vertical, draw D B C. make E H = E G, and H I horizontal and equal to half A C; then if F I fall below

below B, the structure will not give way at the joint A B. The demonstration may easily be deduced from the principle of the equality of the horizontal thrusts in the case of an equilibrium: and it may be shown, that, if the aperture be equilateral, 15 common bricks on each side will stand, but 16 will give way at the sixth joint from the summit. The stability is however less considerable with respect to the second mode of failure, in which the upper brick slides outwards, while all below it fall inwards (Fig. 18). In this case the angular motion of the two portions is initially equal, the points A and B remaining fixed. The velocities of the centres of gravity reduced to a vertical direction are as the distances C D, D E; in order therefore that there may be an equilibrium without friction, the weight of the upper portion must be to that of the lower as D E to C D; and in all cases the force of A D, tending to support D F, is to the weight of D F, acting at its centre of gravity, as A G . C D to F G . D E, or as A G . $\frac{C D}{D E}$ to F G. The friction of the upper block, of which the magnitude may be determined in the manner already shown, will act upon the whole length of the arm F G, while the weight of D F acts only on the length of half D E, consequently its effect must be considered as increased in the ratio of D E to twice F G. Thus if we take the example of an equilateral aperture, constructed with 8 common bricks on each side, and without cement of any kind, C D will be 9·3 inches, D E 2·7, and F G 21; hence the brick A will produce immediately a force equivalent to the weight of 3·4 bricks, and by its friction, which is $\frac{2}{5}$ of its weight, another force equivalent to the weight of 7 more; consequently the sum of both will be fully adequate to the support of the 7 bricks which form the lower portion of the structure. But if we make the same calculation for 9 bricks, we shall find that they will not stand without some external support.

Two modes in which they might give way.

7 bricks would stand by their own weight: 9 would not.

It is obvious that in all these cases the addition of any load at the summit of the structure would very materially increase its stability, and that even a block, of sufficient magnitude to fill up the angle only, would enable us considerably to extend the base. It is also plain, that an inclined facing

This compared with an arch.

facing of this kind is not distinguished from an arch by the want of a key stone, since the two middle blocks act nearly in the same manner as if they were united, except when they are forced outwards by the pressure of the lower parts; and a centre is as necessary for rasing a facing of this kind, as if it were an arch of any other form.

I am, SIR,

Your very obedient servant,

17 *Oct.* 1807. APSOPHUS.

Flattened arches over windows.

Postcript.—The equilibrium of the flattened arches, commonly placed over windows, may be determined in a similar manner, the principles being the same as those which are employed in the construction of Fig. 11 and Fig. 13. Supposing the blocks without friction and of equal height, if their divisions converge to one point, the lateral thrust will be equal throughout, and the whole will remain in equilibrium, provided that the ends do not slide outwards. In order to find the breadth which is within this limit, let the horizontal line A B (Fig. 19) pass through the centre of gravity of the blocks, draw any line C B from the centre of divergence C, make B D = A B, join C D, and let the vertical line B E meet it in E; then E F, drawn to the intersection of the semicircle E F G with the lower termination of the blocks, will show the direction of the abutment *d*, which will afford an equilibrium: and C H parallel to it will determine the greatest breadth that will stand. But since the blocks thus disposed, and supporting a wall, cannot slide away without displacing the superincumbent weight, the whole wall may be considered as adding to the height of the blocks, and the stability in every case that can occur in practice, must be complete: it is only necessary to reduce the horizontal thrust as much as possible, and this must be done by making the point C as near the blocks as convenient: the thrust being equal to the weight of the portion A H, supposing A C H half a right angle. If we wish to estimate also the effects of friction, let the segment E I G contain a right angle diminished by the angle of repose, then C K, parallel to E I, will be the direction of the abutment

abutment which will secure the blocks from sliding outwards, with the assistance of the force of friction. Generally however the obliquity must be much less than this; and the resistance of the abutment becomes capable of being exerted in the most favourable direction that its friction will allow, that is, in a direction more nearly vertical than the perpendicular to its surface, for example L M, M L N being the angle of repose; and if we wish to have the thrust equal throughout, we must employ blocks of such a form that their divisions may make, with the lines converging to C, angles equal to M L N; this however would lead us to make the middle blocks of the form of inverted wedges (Fig. 20), or at least to make their divisions parallel: but it will be sufficient in practice to cause the parts next the abutments to converge to points somewhat nearer than the point of convergence of the middle parts (Fig. 21); nor, indeed, has this arrangement any material advantage over the simpler form of lines converging to a single centre.

Observations on arches in general.

From a consideration of these principles, we may derive some useful inferences with respect to arches in general, especially such as are employed in buildings. The objects to be attained in the construction of an arch are to diminish as much as possible the horizontal thrust, and to secure the stability by such an arrangement as requires the least size in the blocks and firmness in the joints. The size of the blocks must be such, that the curve of equilibrium, under the pressure actually produced by the walls, may be every where included within their substance, and even without coming very near their termination; and the horizontal thrust will be less in proportion as the curvature at the vertex is greater, that is, other things being equal, as the arch is higher. Supposing the height of the wall supported by the arch to be very considerable in proportion to that of the arch itself, the curve of equilibrium must be very nearly a parabola: if the wall is raised but little above the arch, it will approach to a segment of a circle. In order therefore to find whether the size of the blocks is sufficient, describe a parabola through the summit and the abutments; and if it pass wholly within the blocks, they will stand; provided however that their joints are either perpendicular to the curve,

Size of the blocks.

Horizontal thrust.

Curvature.

curve, or are within the limits of the angle of repose on either side of the perpendicular. But if the wall is very low, and the arch flat, a segment of a circle will be more correct than a parabola. Hence it is obvious, first, that a segment of a circle is a better form for an arch than an ellipsis of equal height and span, although less pleasing to the eye, the horizontal thrust being less: secondly, that for the same reason, a Gothic or pointed arch is preferable to a Saxon or semicircular arch, when its height is greater; and even when the height is equal, an arch composed of two parabolic segments meeting in the vertex is stronger than a semicircular arch: for, supposing the wall very high, the depth of the arch stones of a semicircular arch must be at least $\frac{1}{7}$ of the span, in order that the arch may stand, but that of the stones of a Gothic arch, composed of two parabolic segments, may be less by one twentieth; the parabola of equilibrium touching in this case the internal limit of the arch at $\frac{1}{7}\frac{1}{0}$ of its whole height above the abutments. If, however, the arch is flatter, a segment of a circle will be somewhat stronger than a pointed arch composed of parabolic or elliptical segments. When the arch is higher, it is obvious that a single circular curve is no longer applicable: and in this case, it is of little consequence whether the segments be circular or parabolic, either of these forms approaching sufficiently near to the curve of equilibrium, and both producing equally a much smaller horizontal thrust than a semicircular arch.

Circle preferable to the ellipsis.

Pointed arch best in certain cases.

In others the circular.

II.

Additional Remarks on the capillary Actions of Fluids. By ALETES.

To Mr. NICHOLSON.

SIR,

Capillary action of fluids.

IT has been observed, with apparent justice, by Mr. Laplace, that the force of capillary action, other things being equal, must be proportional to the square of the density of a liquid; and it is easy to deduce this result from the

the demonstrations which you did me the honour to insert in your 76th number. The area of the triangle A E C (Vol. XVIII, Pl. I, Fig. 8), which shows the magnitude of the cohesive force at C, is proportional to the square of the line A C, representing the distance to which the force of cohesion extends; and if the same number of particles be condensed into any smaller space, the force will remain the same, and it will still be proportional to the square of the number of particles concerned; or, in other words, to the square of the density of the substance. The same remark is also applicable to the tension of the common surface of two liquids, or of a liquid and a solid; and this determination of the force ought perhaps to have been employed in the investigation of "the angle of contact of a solid with a fluid"; but it is very singular that the result of this investigation will be precisely the same, whether we proceed on the supposition of a tension proportional simply to the difference, or to the square of the difference of the densities. Thus if the density of the fluid C B E (Fig. 6) be called a, that of the solid B, b, and that of a second fluid, supposed to occupy the space C B A, c; if the tension be simply proportional to the difference of density, we may call the force acting in the direction B A, $b-c$, in the direction B E, $a-b$, and the difference of these, $2b-c-a$, must be equal to the force $a-c$ in B C, reduced to the direction B H, and must be represented by the line B H, if $a-c$ be represented by B C or A B, A H being $2b-2c$; or if A E be called $a-c$, A H will be $b-c$. Now if, instead of $b-c$, $a-b$, and $a-c$, we take their squares, the difference of the first two will be $cc-aa-2bc+2ab = 2b(a-c)-(aa-cc) = (2b-(a+c)).(a-c)$, which is to $(a-c)^2$ as $2b-a-c$ to $a-c$, and B H will be to B C in the same proportion as before. It is obvious that when there is only one fluid, and $c = 0$, A H must be to A E as b to a, upon either supposition.

Capillary action of fluids.

The two suppositions are however not indifferent with respect to many other cases of the actions of capillary forces. Thus if two liquids be capable of perfectly wetting a tube, supposing both of them to be contained in it at the same time, the whole weight supported by the force of capillary action

Capillary action of fluids.

The diminution of the effect of the tension of the surface of the drop, in consequence of their obliquity, appears to be exactly counteracted by the force derived from the curvature of the horizontal action; and the film left on the surface seems to occasion a resistance to all motion, which renders it difficult to observe the slight mutual attractions of the drops which must arise from the minute depressions that surround them.

I am, SIR,

Your obedient humble servant,

16 *November.* ALETES.

III.

On a Kind of Death, that may be presumed to be only apparent: by Mr. Du Pont de Nemours. *Read at the first Class of the Institute, Oct.* 28, 1806 *.

Effects of heat vary according to the body to which it is applied.

IT has long been known, that the effects of heat and cold vary according to the nature of the bodies that are exposed to them. Extreme heat is necessary to liquefy steel, platina, or good porcelain: lead requires far less; and a portion much smaller still is sufficient for frozen water. On the other hand, the degree of cold requisite to render mercury solid is very great; while that which forms ice is very moderate.

Some vegetables not killed by the severest frosts

Among vegetables there are many, the living principle of which resists the strongest frosts, these only occasioning disease in them, or, if I may use the term, setting them asleep.

Leaves destroyed by it; and stalks:

Our native trees lose their leaves in winter, without their stems being injured. Many of our herbaceous plants lose their stalks, though their roots retains their functions.

but some roots not, even though frozen.

There are plants still more robust, which, after their roots have been frozen in the ground, into which the frost has penetrated several feet beneath their ramifications, revive notwithstanding at the return of spring.

* *Archives littéraires,* Vol. XIII, p. 3.

If we proceed to animals, we see the ant fall asleep in a very slight degree of cold; and the common fly does the same with every appearance of being dead. Nor are these the only insects subject to this lethargic sleep. Ant, fly, and other insects, sleep in cold weather.

In the class mammalia we have dormice, marmots, and other sleepers, in which life appears to be suspended, when cold weather comes on. This suspension of life is so complete in some of the species, that their heart ceases to beat for whole months. In some of the mammalia life suspended.

The snail and the toad undergo the same stupefaction. Several serpents exhibit a phenomenon still more surprising: they can be frozen so as to become brittle, and die if they be broken in this state; but if they be left in their holes, into which the warmth of spring penetrates by very slow degrees, they revive, and give proof they were not dead. Snail and toad. Serpents may be frozen without being killed.

It is in the season when their food begins to fail, when the fruits and herbs on which they fed disappear, after having fattened them by their temporary abundance, and in this fat supplied them with a narcotic to induce sleep as well as food to support them while it lasts, that most of these little devourers conceal themselves to sleep, and cease to afford prey to the larger devourers their enemies, which in their turn lose thought and motion. Little fructivorous animals sleep first: then the larger & carnivorous,

Those that would be deprived of food by the snow covering it, sleep till the snow melts, and a little longer. Perhaps for a similar reason the white bear, which lives by fishing on the seashore during the summer, and on the islands of ice in autumn, does not fall asleep till the ice united, thickened, and raised too high above the water, is no longer the resort of the seal. His means of subsistence continuing longer, a much severer cold is requisite to deaden in him the call of seeking it, than in the black bear in the first place, a great devourer of honey and vegetables, and next in the brown bear, which lives on animals that winter drives into their retreats before him. They sleep, while their food is wanting. Hence the white bear retires later than the black bear; or even than the brown.

That hunger should cease in these animals at the period when famine would take place, and in consequence of the same degree of temperature, is certainly a great benefit conferred on them by that *Intelligence*, which regulates every Providence has ordered it thus, to prevent them from starving.

part

part of the universe. If they retained their energy, they would perish from inanition. They are unacquainted with want; they feel not its pains; they incur not its danger. Nature saves them from it by that axiom, which has been considered as a jest: he who sleeps, dines. The state of stupefaction, in which their vital principle takes refuge as long as the cold and its companion want continue, occasions them no uneasiness: it commences even with a sensation of tranquil enjoyment, a sensation not unknown to ourselves.

Sleep is a succedaneum for food.

This stupor not painful, but pleasurable.

It is certain, that being thus benumbed is not even a disease: that the drowsiness, which brings it on, is pleasurable: that sleep is an asylum, in which life fortifies itself, expending less, and husbanding its resources: that it is even a protection from the injuries as well as from the pains of cold: and that it renders living bodies more capable of retaining heat, by diminishing their conducting power.

Not a disease, but friendly to life, and renders the body a worse conductor of heat.

When the cold increases with too much violence, man becomes insensible to it. If one of his limbs freeze, he does not perceive it, till he is informed of it by others*. On the contrary, he fancies himself at length growing warmer; and if no one of his limbs be more affected than another, his state seems pleasurable: he feels a seducing and delightsome propensity to sleep: he is angry with his friends, who urge him to walk on, and prevent him from indulging his inclination: he intreats them, to let him close his eyes for a few moments; and if they yield, he falls asleep, and appears dead like a dormouse.

When our limbs freeze, we are insensible of it.

Effects of general cold.

Let us venture to suppose, that he is not more dead in reality. There is no doubt in this case, but he would sleep like the dormouse, deprived of thought and of apparent vital action, at least as long as the same temperature continued. We may even presume, that he would really and completely lose his life at the expiration of a certain time, if he received no succour: for instance, after his fat was consumed, if he were not frozen as well as asleep; or after the habit of the vital functions had been entirely extinguished by a frost too severe, or of too long continuance, so as to stop the alimen-

His death in this case only apparent, like that of other animals:

though it may end in extinction of life.

* This is very common in Poland and Russia.

tary

tary transfusion of the fat, or stiffen the organs to such a degree, as to cause an absolute cessation of their secret movements, which appears to be an accident that the mammalia are liable to, though some of the serpents and gelatinous animals are not.

This important point, that the general vitality of man is merely suspended in this case, is indicated by the repeated experience of northern countries and lofty mountains with respect to the particular vitality of frozen limbs. We may infer this by reasoning from a part to the whole.

It is by no means uncommon for the nose, ears, hands, or feet, to freeze in cold climates. If this be not quickly remedied, the contrast between the living state of the rest of the body and the incipient death of the limb attacked occasions this limb to sphacelate: Nature cuts it off by the strenuous resistance of the contiguous and threatened limb. If hasty means be adopted to remedy it, the too rapid distension of the capillary vessels by the fluids contained in them ruptures their sides, and extravasations take place; the communication, instead of being restored, is thus completely interrupted, and a gangrene is induced from the same cause. The same thing happens to those buds of plants, that are exposed to the rays of the sun, before the frost that covered them has been dispersed; while those that are thawed gradually in the shade receive no injury. "Nature," says the great Newton, "is consistent with herself." In all things, and at all times, she follows the same laws. The more she is observed with a philosophic eye, the more we perceive, that these laws are few, and combined with admirable, with astonishing benevolence. Effect of cold on a single part. Effects on vegetables similar.

It is the same then with the limbs of animals, as with those of vegetables. If they be thawed with cautious slowness; if the part frozen be removed from the extreme cold it has experienced to a less degree of cold; if it be rubbed with snow, then immersed in the water of melting ice, and this be suffered to warm gradually, at first by the mere contact of the contiguous unfrozen portion of the limb, the part frozen will recover its local vitality. The cure may then be completed by very small successive additions of water a little They must be treated by diminishing the cold by slow gradations.

warmer than that by which the part was thawed, and thus the limb be saved.

Similar treatment would probably succeed when the affection is general.

It is agreeable to all analogy to believe, or at least to suspect, that what thus happens with complete certainty to every limb, under such circumstances, must also take place with respect to the whole of the limbs, if the same or still greater precautions were observed, if the delay of succour be not too long, and if its application be not too rapid.

There are many facts in support of this.

Instances of effectual assistance being given to men, either entirely frozen, which however is perhaps doubtful, or at least profoundly benumbed, have occurred frequently in our glaciers, and are mentioned by our colleague Ramond. Haller regrets, that no means were tried with a man, who was thrown up by a torrent of melted ice long after he had been buried under it, at least as far as could be judged by his dress, though his skin was not in the least discoloured.

Portal's method good,

Our learned colleague Portal, in his excellent work on asphixies, points out for that which results from cold a method of treatment founded on very good principles; but I

but too hasty.

am apprehensive it would be found too hasty in its progress, as it is more so than that employed in the case of a single

Should be more cautious than in a mere local affection.

limb, and it may be presumed than a general affection, being more serious and formidable than an affection merely local, must require still more circumspection in the progressive graduation of the means employed: the rupture or even aneurism of a few vessels might have much more fatal consequences in this case.

The path is opened:

Be it as it may, it is evident, that the first steps are already taken toward a constant and complete theory of the effects of cold on plants and animals, in respect to the degree of temperature, that suits each species. But if this new branch of the beautiful stream of knowledge be opened, and begin to excite our attention with advantage, we must con-

but not sufficiently explored.

fess, that it has not hitherto been sufficiently explored; that of the important phenomenon of the life or death of men frozen or simply laid asleep by cold we know nothing certainly, either with respect to the periods or the physiology of the transition from one of these to the other, the succession of means to be employed for the preservation of those whose

lives

ves are thus endangered, or the path to be pursued for the dvancement of our knowledge of them.

Experiments have not been repeated sufficiently or with ue regularity: they have neither been as scientifically di- ected, nor as scrupulously described, as the case requires. t appears to me therefore an object worthy the attention of he class, to point out these experiments; to indicate the roper path of inquiry with a view to improvement; to as- ertain, whether man be in fact, like the bear and the mar- not, an animal that cold benumbs and lays asleep without illing; whether it be true, that he can endure being com- letely frozen like the serpent; and whether in this extreme ase a gently graduated warmth, applied in time, and slowly leveloped, would restore life. This might throw a great ight on the question, which Drs. Herholdt and Rafn have et treated but in part, and the term of which you have pro- onged. Desiderata.

We see that several animals destitute of vertebræ, among hose with vertebræ serpents, and among the mammalia a reat number of the smaller species with cutting teeth, sleep hree or four months, or even more, in a very moderate de- ree of cold: that a greater degree, and this in different pro- ortions, is required for the several species of bears, the sleep f which too, in this state of imperfect death, does not ap- ear to be so profound, or the suspension of life so complete: hat man falls asleep in a still greater degree of cold: that it s more than probable, nay almost certain, that judicious neans, prudently administered, would be capable of awaking im from this dangerous sleep, so near akin to death: and hat it might be the same with other animals, or, indeed, with all animals, that are rendered torpid only by a degree f cold still greater than that which deprives man of the ap- earance of life. Difference in hibernating animals. Man. Perhaps all animals may be rendered torpid by cold, and recovered.

Every particular, even to the minutest, that the respect- able monks of St. Bernard, and the guides to the glaciers of he Alps and Pyrenees, can give, should be collected. I even think that the class would do well, to call the attention f all the learned to this subject, and particularly to invite he four academies of Europe, that are best situate to pur- The minutest particulars should be col- lected.

sue it with success, those of Petersburg, Wilna, Copenhagen, and Stockholm, to investigate this point of natural history.

Wars have nothing to do with the sciences.

To this political circumstances can be no obstacle. There is never war between the learned, between academies. Neither our emperor nor the king of England has disapproved your communicating with the royal society of London for the interests of the sciences. Scientific voyages have been respected by both parties. In the electorate of Hanover the university of Gottingen was protected by our army. The republic of letters, that great and noble benefactress of all polished states, ever preserves its honourable and friendly neutrality.

Suspended animation of drowned and suffocated persons already restored.

Already many of those who happen to be drowned, or suffocated by carbonic acid, are restored to life. These are two provinces, which Humanity has conquered from the empire of Death at the two extremities of his domain: for these two diseases, so long considered as deaths, are totally different in their nature, and require opposite means of cure.

Those princes, who dispute with each other the territories of the living, see with regret by how many murders they must be purchased: they would be the first to encourage the peaceful labours and fraternal correspondence, that might conduce to recover from the yet doubtful bonds of death a third class of its victims, men benumbed by frost.

IV.

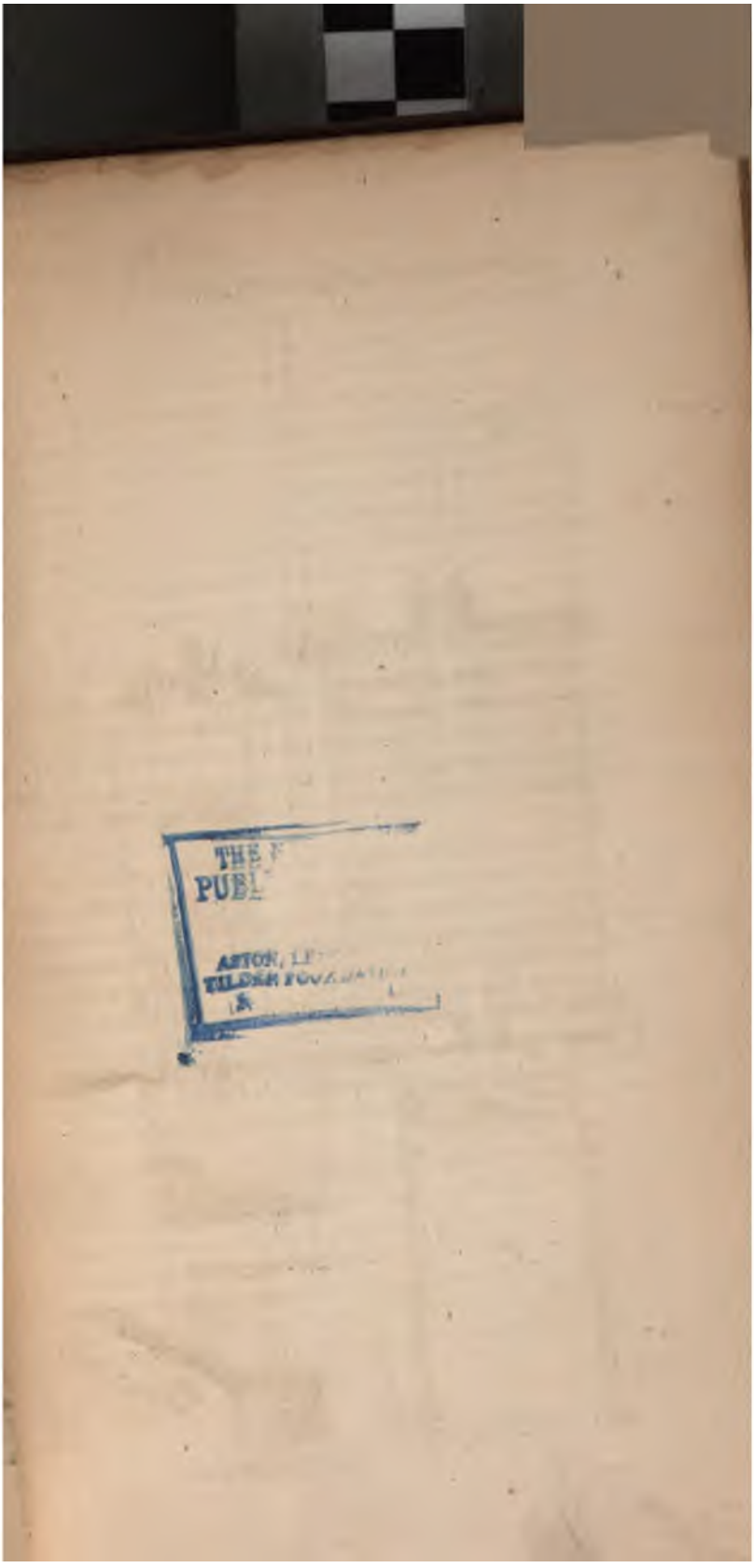
Description of an Engine for affording Mechanical Power from Air expanded by Heat; by Sir GEORGE CAYLEY, *Bart.*

To Mr. NICHOLSON.

SIR, *Brompton, Sept.* 25, 1807.

Expansion of air by heat considered as a first mover.

I Observed in your last vol. p. 368, thatsome experiments have been lately made in France upon air, expanded by heat, applied as a first mover for mechanical purposes. This idea,


THE
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Sir Geo. Cayley's Expansion Air Engine.

Fig. 1.

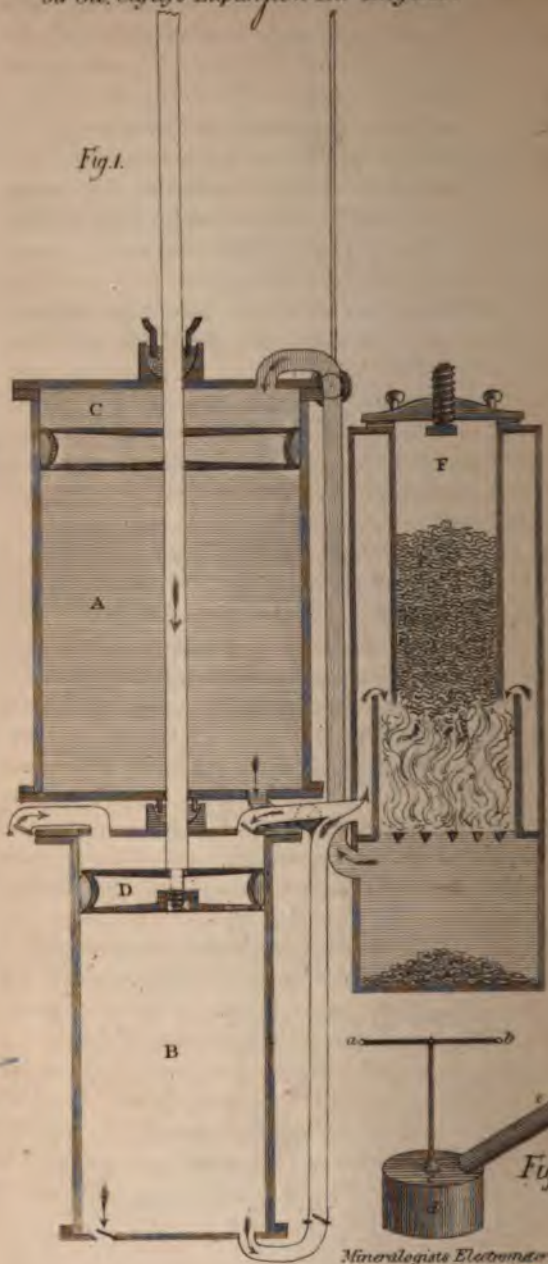


Fig. 2.

Mineralogists Electrometer

idea, as you justly remark, is by no means new in this country; yet I have not heard that any successful experiments have been made, exclusively upon this principle, in England, though you hint that something promising has been accomplished relative to it.

The subject is of much importance, as the steam engine has hitherto proved too weighty and cumbrous for most purposes of locomotion; whereas the expansion of air seems calculated to supply a mover free from these defects. Under this impression I send you a sketch of an engine I projected upon this principle several years ago; it was made on a considerable scale at Newcastle, though I must confess without success in the result, which I attributed to the imperfect manner in which it was executed, the cylinders being made of sheet copper, and so irregular, as not to be rendered tolerably air-tight by any packing of the piston. I think there can be no doubt that the scheme is practicable in some way or other; and I conceive that the form of the engine here sketched will be the basis of whatever experience may prove to be additional requisite to perfection in the apparatus of the air engine. Its advantage

A and B, Pl. VIII, fig. 1, are two cylinders, placed one above another; C and D, their respective pistons connected by one rod. F is a cylinder, containing a fire in a vessel within it in such a manner, that any air passing between the upper and lower portions of it must go through the fire. This vessel also contains a long cylinder, open at the bottom, and directly over the centre of the fire, for the purpose of holding coke or other fuel. This cylinder is covered at the top, and packed air-tight when it has served the purpose of permitting the fire to be kindled through it: and has been filled with fuel. Description of an engine; consisting of a blowing cylinder & a working cylinder.

The cylinder B is fitted up to answer the purpose of a double stroke forcing pump, or bellows, to drive the air into the upper portion of the vessel F, from whence it passes *downwards* through the fire for the purpose of consuming the smoke (the fresh fuel being supplied from the reservoir above) in its passage through the more completely ignited cinders below. In this act the air is expanded; and, by means of pipes from the lower portion of F, it is conveyed alternately

alternately above and below the piston of the cylinder A. In each pipe is fixed a stop cock or valve, so constructed as to open a passage to the external air, when it shuts the connexion with the fire vessel. These cocks are worked by a plug frame.

From this construction it will appear evident, that whatever expansion the air receives, its pressure will operate alike upon the piston of the bellows and of the receiver; and that always in opposition to each other: Hence the power of the stroke will be in proportion to the excess of the area of the receiving piston, over that of the feeding one, multiplied by the expansive force of the contained air, and by the length of the movement.

The engine may be used for gas or decomposed water.

If, when the engine is well constructed, the expansion of the air in keeping up the fire be not found sufficiently sensible, still the form of the engine is such as to admit of either inflammable gas, oil of tar, or other inflammable matters, being injected, each stroke, upon the fire; so that all the heat generated by the united combustion may operate without waste; perhaps even a slight sprinkling of water, either upon, or round the sides of the fire, might answer the purpose. It scarcely need be observed, that a tube connected with a small forcing pump are the only things required for producing these effects.

I remain, Sir,

Your obedient Servant,

GEORGE CAYLEY.

V.

A Letter from Mr. ROBERT HARRUP *to the Editor, on the Diseases of Wheat.*

To Mr. NICHOLSON.

SIR,

Reference to the author's former paper.

IN a former communication on smut in wheat, inserted in your Journal last year, I gave an account of some experiments which proved, that the *principal* cause of the disease

is

is smut mixed with the seed, and that although the diseased grains do not vegetate, they produce smut ears in the crop. It was also shown, that the seed corn prepared with lime prevented the disease from proving injurious in any considerable degree. At that time my observations led me to conjecture, that animalcula might probably be the primary cause. By reasoning from analogy, it still, however, remained doubtful, whether these minute creatures might not be the effect rather than the cause of the disease: I therefore resolved to attempt an analysis of the smut itself.

On the cause of smut in wheat.

From various unexpected circumstances I have been prevented completing the inquiry, and am at *present* only warranted in announcing, that *one* of the component parts of smut is the sole cause of that destructive malady, and that wherever it exists, whether in the seed or in the *soil*, the crop will be tainted. My chief motive in forwarding you at *this time* what may be deemed a premature communication is, to earnestly recommend to all practical agriculturists the following receipt for the preservation of seed wheat.

Receipt for preparing seed wheat.—Soak it well in lime water: then keep it for some hours in lime & water. Dry it with lime powder, and sow.

Put the wheat gradually into limewater*, at the same time carefully taking off the light grains which float on the surface. After standing covered with the water to the depth of five or six inches, and the vessel closely shut during twelve or fifteen hours, stirring it twice or thrice in that time, the liquor is to be drawn off, and the wheat put on a floor. The following mixture is then to be poured regularly over it, viz. Lime, five pounds; boiling water, three gallons: stir them together till the lime is reduced to a powder, which will happen in a minute or two. It is then to be intimately mixed with the wheat, which after lying covered with cloths for some hours may be dried with lime in powder, and immediately sown. The above quantities are sufficient for five bushels of grain, and more ought not to be prepared together.

* Limewater is made by mixing (boiling) water and quicklime together, about one pound of lime (more or less) will be sufficient for three gallons of water; and, after standing an hour or two in a covered vessel, pouring off the liquor, which, if not immediately used, must be kept in a vessel closely shut.

There

An alkali would probably have the effect of lime.

There can be little doubt but either of the fixed alkalis would have the same effect as the lime; but as they are now under trial, I do not venture to recommend them till I am certain of the result.

Other diseases of wheat.

Although smut is the most destructive, yet there are other diseases incident to wheat, which are sufficiently injurious to deserve attention. I shall therefore take the liberty of making a few observations on each of them.

In the second edition of Adams's Essays on the Microscope by Kanmacher a description of animalcular eels is given, in what is there called *blighted wheat*. The grains are said to be blackish, and contain a white soft substance, which separates into numerous filaments when put into water.

Account of the animalcular eels in diseased corn.

Needham was the first who discovered, that each of these filaments was a living creature. He sent some of the grains to M. Folkes, Esq., at that time president of the Royal Society, with an account of his discovery. They were delivered to Mr. Baker for examination, who after repeated trials could discover no other motion than a separation of the fibres or threads, which he imputed wholly to the elasticity in them; and perceiving no token of life, after watching them with due care and repeating the experiments, an account thereof was sent to Needham, who from his own trials found out the cause of failure, and advised him to steep the grains before he attempted to open them. This method proved successful; and at different times after this Baker made experiments with grains of the same parcel, without being once disappointed. He soaked a couple of them in water for the space of thirty-six hours, when, believing them sufficiently moistened, he cut one open, and applying some of the fibrous substance to the microscope in a drop of water, it separated immediately, and presented multitudes of the anguillulæ without the least motion or sign of life; but being taught by experience, that they might notwithstanding possibly revive, he left them about four hours, and then examining them again, found much the greatest number moving their extremities pretty briskly, and in an hour or two more they appeared as lively as these creatures usually

are,

are. These grains were four years old at the time the experiments were made.

M. Roffredi sowed some of the grains, which sprung up; but the ear was either wholly or in a great measure spoiled, being filled with these eels. He also found them in other parts of the plant. In order to disengage which, it must be soaked in water, and then compressed a little. At first sight they seemed to resemble the foregoing; but a more accurate inspection showed, that they were different in structure, and much more lively than those which were procured from the dried grain. They also increased in size in a certain proportion to the plant, so that at last they were observed with great ease by the naked eye, being two tenths of an inch long, and nearly one tenth in diameter.

The disease grain propa gated, and the same k of animalcu

This disease, which I am informed is known by the name of *ear-cockle*, or, in some parts of the country is called *burnt-wheat* from the appearance of the grains, claims particular attention, as it is by no means uncommon, and never fails to prove highly injurious. I saw a field of wheat the present season, in which ears of this sort were so abundant, that it could not be worth above one third of the value had it been clean: and I have heard of several more.

The disea called the cockle, or burnt whe

The cause seems to be either in the seed or in the soil, or perhaps both contribute; but it seems by no means to be influenced by the weather. On a superficial view, the ears have much the appearance of those of smut, but on closer examination are found to be very different. The grains, before they are ripe, are of a dark olive green colour, not exactly the shape of sound grains, and contain a white soft matter, which does not fill them exactly. When at maturity they are black, and by keeping become considerably harder than common wheat, and have much the appearance as if they had been scorched. When they are opened in this state, the black hard crust is found to be about one third of their thickness, surrounding the white substance. The white matter is the anguillulæ, as mentioned before. By viewing them with a magnifier before they are removed, they have somewhat the appearance of very fine cotton fibres coiled together, and no motion is perceptible amongst them. But if a recent grain is opened, and a few of them taken out

Inquiry re specting it cause.

out on the point of a quill, and in that situation examined by a hand magnifier, a slow yet very perceptible motion in every direction may be seen in the extremities of those that do not adhere closely to the main body. Indeed they seem to be incapable of motion in any great degree till put into water, being glued together by a viscid matter. When taken from the recent grain and put into water, they are seen to use every exertion to disentangle themselves, which is generally a work of time.

Life of the animalcula in the grain, &c.

It would appear from these facts, that they enjoy existence while enclosed in their dark cells; and probably the small space which they do not occupy in the grain, and which appears empty, is designed to contain air, which I believe is absolutely necessary to the existence of every creature however minute. It also appears, that when the grains become dry animation is suspended, and continues so till moisture is again added.

Suspension of animal life.

That animal life can be suspended for four years at least, as appears from the experiments of Baker, already noticed, and how much longer we know not, and be then restored by a drop of water, must ever excite wonder and admiration. The reason why Baker failed in restoring life by immediately putting the anguillulæ from the old grains into water seems to be, that the *sudden* application of that fluid was too much for their delicate frames, for when the grains were previously soaked, so that the water gradually penetrated the containing crust, he succeeded.

Grains eight or nine months old do not require to be soaked, as the anguillulæ will revive in a few hours, when put immediately into water.

The author never succeeded in raising any plant from this diseased grain.

I repeatedly sowed some of this diseased grain, but never succeeded in raising a single plant, nor even in producing the least appearance of vegetation. Indeed I had little expectation of success, for the whole of the substance contained in the seed consisted of animalcula, excepting a very minute portion of flocculent matter, which could only be discovered when in water. I took up some of the grains after lying in the ground upwards of nine weeks. Several of them were empty, and others contained a few of the anguillulæ, which moved briskly the instant they were put into water.

water. One of the grains was filled with a brownish substance full of perforations, from which issued small worms discernible by the naked eye. They continued to live in water, and upon examination corresponded in all respects with those seen by Roffredi in the plants which he raised. Since that time I have opened several recent grains, which contained a few of this sort, as well as the anguillulæ. They were more than twice the diameter, and about the same length. Perhaps the grains which vegetated with Roffredi might not have been so much diseased as those I sowed, for that some are only partially infected I accidentally discovered. The first parcel I procured were only blackish on one side, and contained very few animalcula. The rest of their contents appeared similar to that of sound wheat. There can therefore be little doubt, that had they been sown, they would have come up. However that may be, it is certain they do vegetate, for I have since that examined several roots of this diseased wheat, and frequently found the grain from which they sprung to be similar to that in the ear.

but he thinks it possible and probable.

None of the anguillulæ I examined exceeded the two hundredth part of an inch in diameter, and they were in general about twenty-six times that length. When viewed with a hand magnifier, they appear of a silvery white, but when placed in the microscope under a highly magnifying power, they are of a bright chesnut colour. A row of transparent globules, placed at regular distances, run down each side of the body till within a third of the extremities. Last winter I preserved an immense number of them three months in a watch glass, by frequently adding fresh water to them. They did not increase in size, neither were they changed in any respect. If they are not endowed with sight, they are at least very much affected by light. When placed in the rays of the sun, or in the light of a candle, in a minute or two they run together into one or several bunches or knots, and continue so for some time. This effect is most distinctly viewed by means of the solar microscope. When placed in that instrument, they are seen floating from every part of the fluid to form themselves into an apparently inextricable bunch,

Examination of the anguillulæ in the microscope.

They are long lived;

and affected by light.

bunch, and those which arrive last use every exertion to bury themselves amongst their companions.

Many other particulars relating to them might be stated; but as the subject seems not to lead to any purpose of utility, I shall close this account of them by mentioning two facts, which may be of advantage to be known.—*They are instantly killed by adding a few drops of limewater to the water containing them. And if the entire grains are steeped in limewater from twelve to twenty-four hours, the anguillulæ they contain are incapable of being revived, either by placing them in fresh water, or by any other means.* The evident inference from these facts is, that the same preparation which has been recommended for the prevention of smut will also prove effectual for ear-cockle.

The great value and importance of inquiries into the diseases of grain.

An experimental inquiry into the nature, causes, and cure of the diseases of corn would prove a valuable acquisition; indeed it is surprising, considering the importance of the subject, that nothing of this kind has been attempted. Sir Joseph Banks, in his *Short Account of the Cause of the Disease in Corn called by Farmers the Blight*, &c., published in the beginning of 1805, presumes, that the want of actual observation will be abundantly supplied by those, "whose leisure and residence in the country enable them to examine, not only the progress of the crops, but the origin and advances also of all those obstacles which nature has opposed to the success of agricultural labours." We have still to regret, that the expectations of that justly celebrated philosopher have been hitherto disappointed; for I believe little or nothing has been given to the public on the subject since that time.

Two species of fungi in the blight—

From the few observations I have been able to make on the blight, it appears, that the parasitic fungi which occasion it are of several different species, and that none of them are particularly injurious, except the dark coloured, such as that which destroyed the crops in 1804. I was never able to discover, that the orange coloured, which appears early in the summer, was the dark coloured in an immature state.

the yellow and the dark.

On the contrary, I have always found them different; the yellow changing to a dirty ash colour as it approached to decay, and the dark coloured continuing unchanged from its first appearance. In the summer of last year I carefully examined



examined a number of wheat fields at different times, and found the straw untouched by the blight, and in every respect in a healthy state. Every thing continued to have a promising aspect till the heavy rains which fell a few weeks before the commencement of harvest; the dark coloured blight then began to show itself, and continued to spread considerably; and, if we may judge from its progress, had the cutting down the crops been delayed a fortnight or three weeks longer, it would have proved equally destructive with that which took place in 1804. A sufficient number of facts is yet wanting to warrant any conjecture on the manner in which the fungi were produced by the heavy rains; however, we may be pretty well assured, that all diseases which depend so much on the state of the atmosphere, must ever baffle human ingenuity to prevent. The earlier the crops are ripe, the less liable will they be to be injured, and the only remedy at present known is, to cut down the crop whenever the blight begins to make any progress.

Appearance of white and apparently perished plants in a crop of wheat.

Although the above are all the principal diseases of wheat, at least as far as my observation goes, I cannot take leave of the subject without noticing a very common appearance in wheat crops, which is more or less frequent every season, and varies considerably in different fields. Some time after the corn is come out in ear, but yet in a green, unripe state, we frequently observe several plants entirely white, with every appearance of having perished. As harvest approaches, and the corn changes to a bright yellow, these plants, particularly after rain or heavy dews, put on a blackish appearance, as if sprinkled with a black powder. Upon examination by the microscope, this appearance is found to be occasioned by innumerable tuffets of a parasitic plant growing out of the pores of every part of the plant which is exposed to the action of the air, very much resembling some species of the plantulæ of mould; even the sap vessels of the straw are frequently filled in different parts with a black substance, easily discernible by placing the straw between the eye and a strong light. The grains, as might be expected, are small in size, and of a reddish brown colour. Most probably this affection arises from some decay at the root, but whatever that may be I have not hitherto discovered. I have frequently

quently examined the roots with attention, both in an entire state and when dissected, but could never perceive the smallest difference between them, and those of healthy plants.

With the hope that the subject will be further investigated by those whose leisure affords them opportunity, and whose abilities are adequate to the inquiry,

I remain, SIR,

Your obedient humble servant,

Chobham, Oct. 1, 1807. ROBERT HARRUP.

VI.

Description of a simple and convenient portable Electrometer for Mineralogists. In a Letter from a Correspondent.

To Mr. NICHOLSON.

SIR,

Portable electrometer.

LOOKING over Brongniart's Treatise on Mineralogy, lately published at Paris, it appeared to me, that the electrometer he has figured and described for the purpose of detecting electricity in minerals deserves to be better known than I imagine it is; its simplicity rendering it very portable, and always ready to use, without being liable to be out of order. If you entertain the same opinion of its convenience to the mineralogical traveller, perhaps you may find a corner for it in some plate or other of your valuable and useful publication.

I am, SIR,

Your obliged reader,

O. N.

Method of detecting electricity by it.

To discover the production of electricity in a stone by heat or friction, it is brought near to either end of the slender brass needle, *a b*, Pl. VIII, fig. 2, and whatever kind of electricity the stone has acquired, it will make the needle move, if this be done with proper precaution.

But

But to distinguish the kind of electricity developed in the stone, the electrometer must be insulated, by placing it on a cake of resin, and positive or negative electricity may be communicated to it in the following manner. Place a finger on the metallic base *c* of the electrometer; and bring within a proper distance of it a rod of glass, or resin, *e*, electrified by friction. When the instrument may be presumed to be charged with the kind of electricity desired, withdraw first the finger from the base, and then the rod of glass or resin. The stone being then presented to one of the knobs of the electrometer, *a* or *b*; if the stone repel it, the electricity it possesses is of the same kind as that imparted to the electrometer; if it attract it, it is of the opposite kind.

Mode of distinguishing whether it be positive or negative.

Some stones communicate positive electricity to the resin on which they are rubbed. To discover this property, a piece of sealing wax may be flattened on a smooth substance, and the stone rubbed gently on this plane surface. The kind of electricity the resinous matter has received may then be found by means of the insulated electrometer.

Some stones communicate positive electricity to resin by friction.

VII.

A Method of Sowing Clover, and a new Plan for a Rotation of Crops; by Mr. DE VINCENS, *of Thède, near Clermont**.

AT the end of winter, after the hard frosts are over, and when the weather is dry, I sow twenty pounds of clover seed on a *septerée* of land, about 1200 toises [2500 yards] in circumference, sowed with rye the preceding autumn. This seed is harrowed in with a common wooden harrow, which is drawn all over the field by a pair of oxen. Instead of injuring the rye, this harrowing accelerates its growth, and it actually affords a finer crop than rye that has not been harrowed.

Clover sowed on rye and harrowed in.

When the rye is ripe, I cut it in the usual mode; and

* Sonnini's Biblioth. Physico-économique, Oct. 1807, p. 14.

when

when it is carried, the clover forms a green sward intermixed with yellow stubble.

Clover mown in September, and cattle turned on it.

The clover may be mown in September the same year; and cattle may afterward be fed on it till the frosts come on, without inconvenience.

2d year cut three or four times, and then pastured

The second year the clover will be in its most productive state: it may be mowed at least three times for hay, or four for green fodder; beside which it will afford an abundant pasture till the frosts come*.

2d year cut twice, and then ploughed in.

The third year I mow it but twice, and when it has shot up a little after the second cutting, I plow it in with the simple plow of the country. I afterward plow and harrow it repeatedly, till the land is brought to a proper tilth for rye or wheat, which I sow without any manure.

Clover a natural manure for wheat,

It is to be observed, that clover is a natural manure for wheat; that, as is well known, a good crop of wheat may be had any where after a good crop of clover; that the wheat will be so much the better, if the clover have been dressed the preceding spring with 2 cwt. of plaster of Paris to every *quartelée* of 300 toises scattered over the surface.

particularly with plaster of Paris.

Wheat after clover requires weeding.

Wheat succeeds very well after clover without plaster or or any other dressing, but it is indispensable to weed it, and for want of hands I shall in future prefer rye, which when once sown requires no farther care.

Advantages of this practice.

To prove the advantage of my practice in every respect, observe, that on rye † well manured I sow my clover in the spring. This clover costs me nothing either for ploughing or manuring; but only the price of the seed] and the labour of sowing and harrowing it].

A septerée of clover furnishes me, beside the feed at the end of the first and second years, at least five crops of hay during the two years that it wholly occupies the land.

Rotation for corn every other year.

* If the farmer would adhere to the order of cropping where corn is sown every other year, he must plow in the clover after the second mowing of this year.

Rye better than oats with clover.

† I prefer sowing clover on rye to sowing it on oats, in the first place because the crop of rye is more valurble, and in the next because experience has convinced me, that the clover is more forward; no doubt because the rye being already at some height, it germinates and grows more advantageously under its shelter, particularly in dry seasons.

Calculating



Calculating each crop to give me a ton of hay, at £2 10s. a ton, its medium value since the revolution, this would produce me £12 10s. for the two years, at the expense only of the seed, mowing, making, and carrying. Average price of clover hay in France £2 10s. a ton.

To this considerable return may be added the saving of dung for the rye or wheat sown immediately after, and a fine crop of which is certain; if no unforeseen and irremediable accident, as hail or frost, disappoint our expectations. A fine crop of rye or wheat without manure.

After this first crop of corn without dung, I immediately sow rye, manuring it well. Rye.

As soon as this crop is carried off the ground, I sow winter pease immediately on the stubble, covering them in with one single plowing and harrowing. This crop has never failed me: it is earlier than that of corn, and nearly at the same time with winter barley. Winter pease.

When the pease are carried I plow and dung the ground, and sow it with rye. Rye.

After this rye I crop the ground in the spring partly with potatoes, partly with other roots, and the rest with vetches mixed with oats, to be cut as soon as the seed has formed, and employed as pasture. All these crops are previously well manured. Mixed crop.

When the ground is cleared of these, I manure it well, and begin my rotation again with rye, on which I sow clover in the spring, as I mentioned above. Rye and clover again.

If I intended only to sow rye, I should not manure the land, after its having been well manured for the roots and pasturage, and my rye would be the fuller eared. But as the rotation of clover will leave the land three years without dung, I consider this dressing as necessary. The manure necessary on account of the clover.

I shall recapitulate my rotation of crops in the following table. Rotation of crops.

1st *year*. Rye manured: clover sown on it in spring, during dry weather, and harrowed in, without fear of injuring the corn.

2d *year*. Clover in its most productive state. If you would have a crop of clover seed, the second growth this year must be left to ripen. With this view it should be mowed the first time in May or June, when in full flower, and then left to stand for seed.

3d *year.* Clover to be mowed only twice. The third growth to be plowed in for manure.

4th *year.* Rye, or wheat, without manure. The wheat must be hoed.

5th *year.* Rye manured.

6th *year.* Winter pease, sowed on the stubble, and covered by one plowing and one harrowing.

7th *year.* Rye manured.

8th *year.* Vetches, oats, and turnips to be fed off, and potatoes: the whole well manured.

9th *year.* Rye manured, on which clover is to be sown in spring as before.

Observations.

Observations on this rotation of crops.

Corn once in two years.

In the first place it will be seen, that I take care to have a crop of corn once in two years. If my clover interrupt this course by occupying the land two years following, this is balanced by two successive crops of corn after the clover; the first without manure, the second with.

Crops changed to rest the land.

As the land is rested by changing its produce, I do not sow clover on the same land till after an interval of five years.

Crop every year, with manure once in 2 years.

My land produces a crop of some kind or other every year, and in nine [eight] years I manure it but four times. Consequently I have twice as many crops as if it were fallowed every other year, without more expense of manure, and I might almost say without more labour.

Pease do not exhaust the land.

It is particularly to be observed, that, except after the clover, my land has never two successive crops of grain, unless the winter pease be reckoned so, which do not exhaust the land; and that it is so ordered, as to be cropped with corn one year, and with green feed or roots the next. This last rotation interposed between the crops of corn before the clover comes round again, appears to do away the necessity of fallowing, without the assistance of clover, which however I am far from wishing to exclude by this observation.

Fallowing may be rejected without clover.

ANNOTATION.

Remarks on constant crop-

A continual succession of crops without fallowing has a specious appearance of being profitable at first sight; but

as

as an intelligent friend of mine, an excellent practical farmer, observes, a fair estimate of all the advantages of fallowing is seldom taken into the comparative calculation. That land may be brought to bear a crop of some sort or other every year, there can be no doubt; though it is obvious, that precisely the same management cannot suit every species of soil. But when we compute the true value of this practice, we should not reckon from the produce of a few years at first, which will probably be higher than the average at the long run: at the same time we must consider, where a proper rotation of fallows is observed, the saving of seed, of labour in sowing, cutting, inning, threshing, and carrying to market; and the advantage of having the land clean, and reduced to a proper tilth by repeatedly plowing and stirring the soil at times when the cattle and servants of the farm are not required for more necessary labour. Thus when we take into account the certain additional expense on the one hand, to be deducted from the produce of two moderate or perhaps indifferent crops; and on the other the savings in one year, and the produce of a good crop in the next, beside the certainty of keeping the land in heart; we may perhaps be inclined at least to doubt on which side the balance preponderates, in cases where the too sanguine speak decidedly without hesitation. At present it may be presumed no country in Europe can be put in competition with our own for agricultural skill; certainly France cannnot: as however it stands foremost among the useful arts, whatever seems likely to suggest any hint toward its promotion is not undeserving of notice, from whatever quarter it may come.

ping compared with occasional fallowing.

VIII.

A Memoir on Roman Alum, compared with different Kinds manufactured in France; by Messrs. THENARD *and* ROARD. *Abridged by Mr. Bouillon-Lagrange*[*].

THE art of manufacturing alum originated in the East, and remained for a number of years the exclusive property

History of alum works.

[*] Annales de Chimie, vol. LIX, p. 58, July, 1806.

of some cities in Syria. In the 15th century it was brought into Europe, and soon became common in Italy, where that of Tolfa required great reputation by the constant uniformity of its product, as well as its purity. But this art, still in its infancy, was very slowly improved; and it was not till three hundred years after, when chemistry was sufficiently advanced to discover the intimate nature of substances, that it made some progress. Margraff, Monnet, Erxleben, and Bergman, then analysed all the kinds of alum most generally known. Bergman in particular was so well aware of the importance of the question, that he wrote a dissertation of considerable length on the history, preparation, analysis and purification of alum; in which he lays particular stress on the necessity of carefully separating the iron from it by repeated crystallizations, by means of which he says he manufactured alum even purer than that of Rome. He had some erroneous ideas however, which modern chemists have corrected.

Mistake of Bergman.

Potash necessary.

Mr. Chaptal first perceived Bergman's mistake in proposing to saturate the acidulous solutions with clay; and the simultaneous discoveries of Decroissilles, Chaptal, and Vauquelin, on the action of potash in the formation of alum, and on the various combinations of the sulphuric acid with alumine, left us nothing more to wish on these heads.

The knowledge thus acquired gave rise to several alum works, the produce of which, though approaching that of Tolfa, was not able to diminish the preference given it by all manufacturers, or to lower the price it bore. The learned awaited with impatience the solution of this important problem, when Mr. Vauquelin made known the result of his analyses of Roman alum compared with that of some other kinds most generally known. He showed, that the proportion of the constituent principles of alum is always the same, and that they differ only in consequence of a few particles of sulphate of ammonia and of iron, which he could not find in any appreciable quantity in Roman alum. He concluded his interesting analysis by saying, that, if there were so much difference in alum as the dyers say, chemistry in its present state was not able to detect the cause; but that it appeared to him more natural to suspect them of exaggeration:

Vauquelin.

Alum a constant salt,

but frequently contaminated with ammonia and iron.

Preference given to Roman alum by the dyers supposed to be without cause,

ration; and he concluded, that any alum, free from iron, would be as good for use as the Roman. To place this beyond question however, it would be proper to make comparative experiments with them in dyeing.

Encouraged by this some skilful manufacturers farther improved the produce of their works, and supplied the shops with alum, that wanted only a different name and appearance to rival the Roman.

Factitious alums made to pass for Roman.

But the predilection for Roman alum was soon abused; and considerable quantities of the alums of Liege and Javelle, to which all the outward appearance of that of Tolfa had been given, were sold. Most of the dyers and manufacturers however, who at first had been imposed on by this appearance, were induced afterward to be only so much the more eager for the true Roman alum: for it was much more easy to deceive than to convince them.

Prize offered by the Society of Encouragement.

Such was the state of our knowledge repecting alum, when the Society of Encouragement, ever animated with a desire of giving our own manufactures a great preponderance over those of foreign countries, thought fit to offer a prize for the means of giving our alums all the properties of that of Rome. The society having employed Messrs. Thenard and Roard, to compare the Roman alum with that of French manufacture*, in order to ascertain the difference both of their nature and effects; these gentlemen, after having acquainted the society with the results of their inquiry, submitted them to the Institute, before whom they laid the numerous experiments they had made to solve the question.

Directed a comparative examination of French and Roman alum.

Their precautions.

They were very careful to obtain the French alums in the state in which they are commonly sold, and accordingly procured them themselves either from the manufacturers or from the warehouses, taking at a venture a great variety of crystals from among considerable heaps.

It was of particular importance likewise, that they should procure unmixed Roman alum. Accordingly they applied to Mr. Schlumberger, their colleague, who has the care of the warehouse at Paris on account of the proprietors, and to

* Messrs. Thenard and Roard say nothing of the English alum, though it appears from Vauquelin's paper, that the French consumers give it a decided preference over any made in France. T.

whom

whom all the Roman alum is directly sent. Accordingly he had a great number of casks opened, that they might examine the external appearance, figure, and colour of the crystals; and from each they took what they judged proper, to make up in the whole the weight of 30 kilogrammes [about 67lbs.] The superiority of the Roman alum over all other kinds met with in the shops being the object of the dispute between the chemists and manufacturers, Messrs. Thenard and Roard conceived, that to decide it an analysis on a large scale alone would be insufficient; and that it was particularly necessary, to make numerous and very accurate experiments with the best known colouring drugs on the fabrics most in use: and they conceived, that if, from the whole of the facts, they could discover any necessary and direct connexion between the results of the analysis and the practical experiments, between the principles found by the one and the effects obtained by the other, all the difficulties would be elucidated, all doubts removed, and theory conjoined with experience would lead them to a complete solution of the question.

Plan of their proceedings.

Alums compared.

The French alums subjected to their researches comparatively with the Roman were those of Bouvier, Liege, Javelle, and Curaudau.

Before they compared the effects of these various alums in dyeing, their first care was to subject them to all the analytical trials already made by the chemists we have mentioned: thus at the same time they determined the proportions of acid, alumine, potash, and water, and observed, as Bergman, Vauquelin, and Chaptal had done before them, the dangerous influence of iron. The experiments they made on this subject constitute the first part of their memoir.

Part I.

Analysis.

Analysis of Alums.

For the sulphuric acid.

Exp. 1. To determine the proportions of sulphuric acid, they dissolved in 16 litres [or wine quarts] of water 489 gr. [15 oz. 6 dr. troy] of each of the preceding alums entirely freed from the dust that covers the surface of some of them*.

* The rosy dust on the Roman alum yielded on analysis saturated sulphate of alumine and potash, silex, and oxide of iron.

In

Into the limpid solution of each, when completely dissolved, they poured muriate of barytes to saturation, and even added a very slight excess, that they might be certain all the sulphuric acid was thrown down. Each of the solutions required precisely the same quantity of muriate of barytes. The precipitates were washed in 90 quarts of water; and when that of the last washing was rendered but very slightly turbid by nitrate of silver, as the water used for the purpose itself was, they were collected with the greatest care.

After being dried, and calcined at a red heat for an hour, the weight of the sulphate of barytes produced was:

No.		grammes.
1,	Roman alum	489·42
2,	Alum of Bouvier	490·70
3,	——— Liege	490·27
4,	——— Javelle	490·27
5,	——— Curaudau	488·23
	Mean of the whole	489·63

Messrs. Thenard and Roard adopted the proportion of 26 per cent of sulphuric acid in sulphate of barytes, because it is the mean between the results of the analysis of this sulphate obtained by one of them, and those found by Mr. Berthollet after experiments made with the greatest care.

The determination of the proportion of sulphuric acid being the most important experiment, they attempted it a second time with as much precision as before, and found no difference between the quantities of sulphate of barytes obtained by the two analyses.

Exp. 2. The equal quantities of sulphate of barytes obtained by Messrs. Thenard and Roard in the preceding trials leaving them no doubt with respect to the proportions of sulphuric acid in the alums they had examined, they did not think it necessary to analyse any but those of Rome, Bouvier, and Liege, for the purpose of ascertaining the proportions of the other principles. These give us one artificial alum, and two native alums, of which one is the most common, the other the most esteemed. Of each of these 489 grammes well

For the alumine.

well powdered were dissolved by heat in 16 quarts of water, and decomposed by equal quantities of ammonia, which was added in very great excess. The alumine precipitated was washed with 60 quarts of water; and when that of the last washing ceased to precipitate muriate of barytes, it was collected, and dried in a large silver basin. After being dried, and kept at a red heat for an hour, it weighed:

No.	grammes.
1, Roman alum	60·92
2, Alum of Bouvier	61·82
3, ——— of Liege	61·02

Thus Messrs. Thenard and Roard found in these alums exactly the same quantity of alumine; for the trifling differences observed between them do not amount to a gramme [15 ½ grains], and are such as could not be avoided in such a long series of operations.

The authors took so much care in washing the alumine, and not pouring off the water till the sediment was completely formed, and had left it perfectly clear, that they cannot fear having assigned the quantity too small. Neither can it be too great, since, when it was dissolved in nitric acid, the solution did not render muriate of barytes turbid: it was completely freed from any sulphate therefore, that might have increased its weight.

For the potash. *Exp.* 3. The 60 quarts of lixivium produced by washing each of these alums were evaporated to dryness in a silver bowl, and the products obtained were boiled several hours with an equal weight of quicklime. The residuum was treated four times successively with boiling water, to take up completely every thing soluble; and these waters were evaporated to dryness, the residuum dissolved in a very small quantity of distilled water, and this repeated alternately several times, in order to separate completely the last portions of sulphate of lime. The solution of each of the sulphates of potash was evaporated for the third time, and at length heated red hot in a platina capsule.

The

The weights of the sulphate of potash thus obtained were:

No.	grammes.
1, Roman alum	77·05
2, Alum of Bouvier	76·80
3, ——— Liege	77·33

These sulphates no longer gave any sensible precipitate with oxalate of ammonia, and rendered nitrate of silver but very slightly turbid. They contained a little excess of alkali, but in so small a quantity, that a few grains of sulphuric acid were sufficient to saturate it.

Messrs. Thenard and Roard preferred treating the sulphates with lime to employing calcination, for they had satisfied themselves that by calcination acidulous sulphate of potash can only be obtained, part of the alkali always flying off.

Constituent principles of sulphate of potash.

The analysis of the sulphate of potash, repeated several times following, constantly afforded them the same results, and showed, that a hundred parts of this salt consist of

Sulphuric acid	36·4
Potash	63·6
	100

Analysed for ammonia, but none found.

Exp. 4. Desirous of knowing whether the alums they had analysed contained ammonia, they treated them with caustic potash, and with lime; and as they obtained none by this method, they heated them strongly in a retort with an equal weight of powdered quicklime; but they could not thus discover the slightest trace of it. In fact, say they, we should have been surprised, if we had found any, for we knew to a certainty, that it could not be one of the constituent parts of the artificial alums we examined; and as to the native alums of Liege and Rome, as no urine is added in their preparation, the ammonia must have existed in the ore, and from this it must have been expelled by the roasting.

May exist in some alum.

We must not however conceal, that it is possible to find alums with an ammoniacal base, though they must be very rare, for the practice of saturating the excess of acid in the aluminous lixivia by means of urine has been very confined, as

as it has generally been supposed, that this alkali would injure the beauty of dyes.

Analysed for iron.

Exp. 5. The presence of iron in alums had been demonstrated in a positive manner by several eminent chemists, who considered them all, including the Roman, as one and the same salt, except so far as its properties were altered by foreign matters, and particularly by sulphate of iron.

Mode of estimating its quantity.

To ascertain its influence, it was necessary to know the quantity contained in the alum: but analysis not affording any means of determining it with sufficient accuracy, Messrs. Thenard and Roard had recourse to the synthetical plan. Accordingly they took some alum perfectly free from iron, to the solution of which they added from $\frac{1}{100}$ to $\frac{1}{1000}$ part of sulphate of iron; and then they compared the effect of prussiate of potash on each of these solutions, more or less ferruginous, with that it produced in solutions of the five kinds of alum.

Properties.

By this method they found, that the alum of Liege contained at most $\frac{1}{1000}$ of sulphate of iron, that of Javelle a little less, that of Bouvier and of Curaudau $\frac{1}{1500}$ or $\frac{1}{1000}$, and the Roman scarcely $\frac{1}{1000}$.

Component parts of alum.

From all these experiments it follows, that the alums of Rome, Bouvier, Liege, Javelle, and Curaudau contain precisely the same quantities of sulphuric acid, alumine, potash, and water, and differ only by a few thousandth parts of sulphate of iron: and that a hundred parts consist of

Sulphuric acid	26·04
Alumine	12·53
Potash	10·02
Water	51·41
	100

Part II.

Experiments with Dyes.

Experiments with dyes.

After having given the results of their analyses in the first part, Messrs. Thenard and Roard proceed to the second, which includes all their experiments with dyes. As this does not

not appear to us capable of being abridged, we shall give it entire.

Convinced, say they, by the preceding experiments, that the alums of which we have spoken are formed of the same quantities of sulphuric acid, alumine, potash, and water; and that they may be considered as identical, differing only by a thousandth part of sulphate of iron, we began with examining, whether their action in dyes were as different as is commonly asserted. Desirous that this part of our labours should not be inferior in precision to the former, we endeavoured to remove every cause of uncertainty that might occur either from the mixture of the colouring matters or the substance dyed, the variations produced by the time or vessels employed in the application of the mordant, the unequal body of liquor, or the difference of temperature in the baths of dye. As we were anxious to observe with the greatest care all the effects, that might present themselves in the course of our experiments, we performed the greater part of them ourselves, and all the rest were executed under our inspection in our own dyehouse.

All the alums identical, except from the sulphate of iron.

We have not laid before the Institute the results of more than five hundred experiments with dyes that we have made, the greater part of which served only to point out our course, or confirm facts we had already observed: all those we have suppressed would have added nothing to the various proofs we set before them.

All our researches were made at the Gobelins: we could not choose a dyehouse more convenient, or offering us more advantages; for the processes there constantly carrying on, to supply the demands of three imperial manufactories, enabled us to make without interruption very numerous and varied experiments, which could not have been executed elsewhere without considerable expense. There we found every thing we wanted, whether of vessels, dyes, or matters to be dyed; and no where else could we have been assisted by a more able dyer than our foreman, Mr. Blondeau, who to great skill in colours adds very extensive practical knowledge.

ART.

ART. I. *Comparison of the effects obtained in dyeing with the alums of Rome, Bouvier, Liege, Javelle, and Curaudau.*

Comparative trials with wool, silk, linen, and cotton.

The materials we employed in operating with these five alums were wool, silk, thread, and cotton. Each of these we subjected to the preliminary preparations adopted in the most celebrated dyehouses. Aware of the extensive use of

With printed cottons.

cotton for printed goods, and that Roman alum is employed exclusively for all their delicate colours, we were desirous of making some trials, that would enable us to decide upon this subject. We then had recourse to Mr. Davilliers, who readily, and with the greatest politeness, made a trial of our five alums in his manufactory. The patterns he was so obliging as to send us agreed very well with our results; but as the unequal application of the mordant might with some plausibility have been objected to us, we endeavoured to obviate this by adopting another method, that used in dyeing piece goods.

Mr. Berthollet, jun., who has already distinguished himself in the science and in its application to the arts, particularly with respect to printed calicoes, which he has studied with great care in the fine manufactory of Jouy, had the civility to come and direct us in this important part of our labour, and assist us in all the researches we made on this subject.

List of experiments.

Each of the experiments that compose this article was made with all the five alums.

Woollens.

Exp. 1. Weld yellow.
2. Cochineal.
3 & repeated. Madder.
4. Kermes.
5. Archil.

Thread.

6. Weld yellow.

Cotton Thread.

7. Weld yellow.
8. Madder.
Exp. 9. Sumach.

Calicoes.

10. Weld yellow.
11. Madder.
12. Sumach.

Silks.

13. Weld.
14. Crimson.

Silks with the acetates produced from the five alums.

15. Weld.

By



By these experiments we find, that the five alums act generally in the same manner on woollens, that they produce some difference in cotton, and that their effects differ greatly on silk. But these alums contain precisely the same proportions of the same principles, and differ only by $\frac{1}{1000}$ of sulphate of iron: we are therefore obliged to conclude, that the differences mentioned must be ascribed to this sulphate.

No difference on wool, little on cotton, great on silk.

The following are the experiments we made to establish this fact.

ART. II. *Alums of Rome, Bouvier, Liege, Javelle, and Curaudau, in their common state, compared with the same alums purified.*

After having freed these five alums from all the iron that existed in them, we made comparative trials with them thus purified, and with Roman alum and the alums of French manufacture.

The differences owing to the sulphate of iron.

We first employed prussiate of potash to precipitate their iron; but as this method was slow and expensive, we substituted the more simple and well-known process of dissolving the alum in boiling water, and washing the pulveriform crystals in cold water. In this way we obtained the complete separation of all the sulphate of iron from our most impure alum, which then was no longer perceptibly affected by prussiate of potash, even after several days exposure to the air. So complete a purification however is altogether unnecessary for the purposes of the arts.

Table of experiments.

Wool.

Exp. 16. Weld yellow.
17. Cochineal.
18. Madder.
19. Kermes.

Thread.

20. Weld.

Cotton thread.

21. Weld.
22. Madder.
Exp. 23. Sumach.

Calicoes.

24. Weld.
25. Madder.
26. Sumach.

Silks.

27. Weld.
28. Cochineal.
29. Fustic.

With

General effects of the iron.

With weld and cochineal, which are colouring matters the most sensible to the action of sulphate of iron, the purified alums gave us colours more brilliant, fresh, and in a slight degree lighter; while those with our common alums were all duller, and evidently of a deeper hue. This slight increase of the intensity of the colour arises solely from the small quantity of sulphate of iron found in our common alums. To satisfy ourselves of this, we added to our purified Liege alum scarcely appreciable quantities of sulphate of iron, and gradually more and more; till, by thus restoring all it had lost in its purification, we caused it to assume the different states of Roman alum, that of Bouvier, Curaudau, and Javelle, and lastly its original state of Liege alum.

ART. III. *Comparison of the alums of Rome, Bouvier, Liege, Javelle, and Curaudau, in their ordinary state, with the same alums, to which we had added increasing proportions of sulphate of iron.*

Comparison of the alums with additions of iron.

We were convinced to demonstration, that the slight differences produced by these several alums in dyeing were owing to the different and scarcely calculable quantities of sulphate of iron they contained: but to remove completely every doubt, that might still be entertained in this respect, we confirmed by synthesis all the facts, that we had collected from analysis.

The substances to be died, woollen, linen, and cotton, were prepared with solutions of the alums purified, of the same alums with the addition of $\frac{1}{100}$, $\frac{1}{50}$, $\frac{1}{25}$, $\frac{1}{12}$, $\frac{1}{8}$, and $\frac{1}{4}$ of sulphate of iron, and of sulphate of iron alone.

The silks were alumed in the same proportions with the five different alums, and with pure alum to which from $\frac{1}{1000}$ to $\frac{1}{100}$ of sulphate of iron was added.

Table of experiments.

Wool.

Exp.30. 31. 32. Weld.

33. 34. 35. Cochineal.

Exp.36. 37. 38. Madder.

39. 40. 41. Kermes.

42. Prussiate of potash.

Exp.

Thread.	*Silks.*
Exp. 43. Weld.	Exp. 50. Weld.
Cotton.	51. Cochineal.
44. Weld.	52. Fustic.
45. Madder.	*Silks.*
46. Sumach.	53. Weld.
Calicoes.	54. Cochineal.
47. Weld.	55. Fustic.
48. Madder.	56. Weld.
49. Sumach.	57. Cochineal.

From these experiments it appears, that weld yellows are greened and deadened by sulphate of iron. That cochineal is turned violet by it, without being altered so quickly as kermes, or even as madder: and that, without being made dull its colour is sufficiently heightened for persons not much used in comparing colours to prefer generally on wool those produced by Roman alum with $\frac{1}{75}$ of sulphate of iron to those of the pure Roman alum. General effects.

In the colours on cotton, whether sumach or weld yellow, or madder red, notwithstanding the slight differences from the drying of the mordant, experiments 44, 45, and 46, with purified Liege alum and $\frac{1}{150}$ of sulphate of iron, never afforded us deeper or duller colours than the same experiments with Roman alum and $\frac{1}{150}$ of sulphate of iron. On cotton.

Sulphate of iron acts in a more striking manner on silks, for the weld yellows and cochineal crimsons on them were more affected by $\frac{1}{150}$ of sulphate of iron, than on woollens by $\frac{1}{75}$. On silk.

Knowing the great sensibility of silk in manifesting the smallest quantities of iron, we employed it to satisfy ourselves whether our alums did not contain above $\frac{1}{1500}$ part, as we had found synthetically by pouring prussiate of potash into solutions of pure alum, afterward altered by greater or smaller quantities of sulphate of iron. The quantity of iron tested by silk.

We alumed silks with alum freed from iron, Roman alum, the alums of Bouvier, Liege, Javelle, and Curaudau, and similar quantities of pure alum, to which we had added from $\frac{1}{3000}$ to $\frac{1}{150}$ of sulphate of iron.

After

After they were dyed, we found in the series of tints experiments 56 and 57, produced by our pure alum rendered more or less ferruginous, colours perfectly similar to those of our ordinary alums. Thus $\frac{1}{200}$ part of sulphate of iron added to this pure alum, afforded us with weld and cochineal the same colours as Roman alum; $\frac{1}{300}$ the same as the alums of Bouvier and Curaudau; $\frac{1}{400}$ the same as that of Javelle; and $\frac{1}{500}$ the same as that of Liege.

The different effects of alum owing to iron.

We can no longer therefore ascribe the differences we obtained in dyeing with different alums to any other cause, than these infinitely small quantities of sulphate of iron; since, by adding this substance, we converted purified and Roman alum into alums, which gave us the same results with reagents as the most impure kinds of the shops; and, on the contrary, by abstracting the sulphate of iron, we could make at pleasure, from the most impure kinds, alums producing as fine or finer colours than those obtained with Roman alum.

ART. IV. *Experiments on the influence of sulphate of ammonia, and of alum with an ammoniacal base.*

Experiments with sulphate of ammonia.

Many distinguished chemists have asserted on the authority of Bergman, that alums with an ammoniacal base are injurious in dyeing. To ascertain whether this opinion were well founded, we treated wool and silk with several proportions of sulphate of ammonia, which we added to Roman alum, and with alum without potash, having its base entirely of alumine and ammonia.

It produced no effect,

$\frac{1}{100}$ and $\frac{1}{50}$ of sulphate of ammonia produced no perceptible change in silk or wool with weld or cochineal colours.

unless in considerable quantity.

$\frac{1}{25}$, $\frac{1}{12}$, $\frac{1}{6}$, and $\frac{1}{4}$ of this salt produced a regular degradation, in which the colour with Roman alum and $\frac{1}{4}$ its weight of sulphate of ammonia was two or three shades weaker than that with Roman alum alone.

Common alums not injured by this.

Hence we had reason to expect evident changes from alum with an ammoniacal base, but we found no difference in its effect and that of Roman alum.

Wool

Wool with sulphate of ammonia.	*Silk with alum having an ammoniacal base.*
Exp. 58.....Weld.	
59.....Cochineal.	Exp. 60.....Weld.

Such are the facts we were desirous of laying before the Institute, relative to the long undecided question of the superiority of Roman over all other alums. They afford us an exact and complete coincidence between the results of our analyses and of our experiments with dyes: they show us, that much too extensive an action has been ascribed to the sulphate of iron, the whole of the influence of which we have pointed out, at the same time marking its limits: and in particular they prove, that the opinion of the exclusive advantages of Roman alum, formerly perhaps sufficiently just, is now to be considered as an error successfully combated by theory, and demonstrated by experiment. These facts lead us directly to the following consequences. General deductions.

1. All alums contain precisely the same proportions of sulphuric acid, alumine, potash, and water; though they produce sensible differences with reagents, and in their application to the art of dyeing. Alum itself identical.

2. These differences arise solely from the unequal quantities of sulphate of iron found in them, amounting merely to a few thousandth parts, for they disappear completely on the purification of the alums, and are reproduced with the same intensity, if we restore to them as much sulphate of iron as had been abstracted. Differs from being contaminated with iron.

3. The Roman alum contains the least sulphate of iron: the alums of Bouvier and Curaudau afford us a little more, but the quantity they contain is appreciable only by reagents, and on silk in weld and cochineal colours. In the alums of Javelle and Liege prussiate of potash immediately indicates the presence of sulphate of iron. Roman alum freest from it.

4. Roman alum does not merit the exclusive preference given it over other alums, for we have obtained on wool, cotton, and silk, with Liege alum purified by means of water, and even with the alums of Bouvier and Curaudau, as fine and brilliant colours as those produced by Roman alum: and if the latter appeared to us to have the advantage over Other alum may be made equal to it.

the alums of Bouvier and Curaudau, we can affirm, that the differences were very trifling, and only to be perceived by an experienced eye.

$\frac{1}{1000}$ part of sulphate of iron injurious.

5. The alum of Javelle, and that of Liege in particular, though not containing above a thousandth part of sulphate of iron, almost always produce duller and less bright colours than those with purified or pure alums.

Its effect greatest on silk,

6. The effect of sulphate of iron is not the same on all substances, and with all colouring matters: it is very evident on silk with weld and cochineal colours: it is a little less so on cotton, and it is much less on wool, with the same substances. Wool appears to fix a less quantity of sulphate of iron than cotton, and particularly than silk; for the colours on wool are less altered by $\frac{1}{25}$ of this sulphate than on silk by $\frac{1}{100}$; and in all madder, archil, and kermes colours, very large proportions of this substance are necessary to alter the shade, or even to diminish its liveliness.

least on wool.

Little with madder, archil, or kermes.

Every manufacturer may make alum of the best quality.

7. Every manufacturer of alum therefore, if he will, may change his most impure alum, by simple and not expensive means, into an alum, that in its application in all the arts, to the most lively colours, and to substances the most sensible to the influence of sulphate of iron, shall possess all the properties of the long boasted Roman alum.

Remarks on the French trade in it.

Let us hope, that the importation of foreign alum into France, which amounted a few years ago to several millions of livres, and which has already decreased in a considerable degree, will soon cease entirely: that our alum manufacturers, better acquainted with their real interest, will no longer endeavour to distinguish their goods by that coloured coating, which has most frequently been the resource of fraud: that their endeavours to furnish the shops with an alum constantly pure will soon lead all our manufacturers to think no more of Roman alum: and that ultimately our alum works, obtaining deserved celebrity, will be greatly increased, extend their sale to foreign countries, and enrich France with a considerable branch of trade.

IX.

Essays on the Improvement of Pottery in general, or the Art of making, at the least Expense, Vessels for every Use, more handsome, strong, and wholesome, without employing Lead or Tin, in the Composition of the Coating, Enamel, or Glaze: by Mr. C. R. JOUSSELIN, *Manufacturer at Nevers. An Abstract by Mr.* GUYTON *.

IN the pamphlet that has just appeared under this title, the author, after noticing the importance of this branch of industry, takes a rapid view of the periods when the porcelains of Japan and China arrived in Europe, the introduction of Delft ware into France, which dates no farther back than the 15th century, the extensions of the art which have long rendered it an advantageous branch of foreign trade, the importation of the white English ware, and manufactories established to imitate it. History of the art.

He next lays it down as an established principle, at least among men of science or those acquainted with the art, that there is no truly good pottery but stoneware and porcelain: as they are the only kinds, in which strength, neatness, and wholesomeness are combined. No good pottery but stoneware and porcelain.

Before proceeding to the proofs of this, Mr. Jousselin justly observes, that this principle is not only interesting to the progress of the art; but that it merits the greatest attention in a political and commercial view, particularly as the neglecting it must render the French tributary to foreign nations, and this to no small amount, were it merely for the lead and tin employed in the composition of the coatings. Importance of the art to France.

Common pottery, intended to stand the fire, has a very porous biscuit, which is but slightly baked, that it may be capable of sustaining the transition from heat to cold, and because it is not refractory enough to support a greater heat. For the same reason it can only have a very fusible covering. This is commonly sulphuret of lead, and oxides of copper, iron, and manganese. Common pottery for culinary uses. Its glaze.

* Annales de Chimie, Vol. LXII, p. 213, Mai, 1807.

Delft. Delft ware, which was a grand invention in its time, on account of the beauty of its coating, has likewise the defect of being baked only so far as to vitrify the enamel, as a degree of heat beyond this would spoil it. This renders it necessary, to employ a sufficient quantity of lime in it, to give it a little consistency by a commencement of fusion.

Its defects. Its coating, composed of glass of lead and silex, rendered white and opake by oxide of tin, cannot support changes from heat to cold, and its biscuit is liable to imbibe grease.

Queen's ware. The white or *pipe* ware, after the English fashion, is lighter: its biscuit has more solidity, being composed of purer clay, and prepared flints; and it is previously baked: but the coating given it is much more fusible than that of delft; it is a glass, incapable of enduring an equal heat; is subject to crack; is very easily scratched, when any oily matter will penetrate the biscuit and leave spots; and if the glass of lead be in excess, which is unfortunately a too common case, oils and vegetable acids attack it, and render its use dangerous.

Its glaze defective and dangerous. The memoir published by an able chemist, Mr. Proust, to remove any apprehension of injury from its use, induced Mr. Gay-Lussac and myself, to pay great attention to this subject, at the time of the last exhibition of the products of French Industry. We found very little, that was capable of completely resisting the edge of a knife; and after this it could not stand the test even of boiling acetic acid, or the yolk of egg boiled hard. We cannot therefore avoid adopting the opinion of Mr. Jousselin, that, whatever attempts be made to improve this manufacture, it can never form good pottery.

Is stone-ware capable of being made a substitute for every thing but porcelain? Hence then it may be admitted as a general principle, that only two kinds are admissible, stone-ware and porcelain. But is it possible, to answer every purpose of strength, elegance, wholesomeness, and economy, in a word, to render stone-ware capable of supplying the place of common earthen-ware, or such as is required to stand the fire, of delft, and of *pipe-ware?* Mr. Jousselin affirms, that he is convinced it is by numerous experiments. As he is establishing a manufactory, it is natural for him to keep secret

Yes.

the



the processes he has discovered, though what he says appears sufficient, to give him a claim to our confidence.

Without attempting to divine his secret, I shall add in support of its possibility, that it might reasonably have been questioned, when the art was but a traditional practice, and when all our stone-ware was of a close texture, incapable of supporting the fire without cracking: it might have been questioned before the experiments of Lauraguais in 1762, whence Mr. Jousselin dates the first conception of a common porcelain, and whose successes did not meet the encouragement they deserved: before the property of magnesia to put a stop to the fusion without imparting any colouring principle, and that of barytes to supply the place of saline fluxes, were known: before the analyses of feldtspar had taught us to compose it artificially with very common materials: before the property of pumice stone to afford a covering not attacked by any menstruum was discovered; and before the inventor of this process, Fourmi, crowned by the Institute in the year 12, had fabricated his *hygiocérames*, a species of common porcelain capable of standing the fire: before the effects of heat prolonged to devitrification had been observed: and before the productions of the manufactories of Utschneider, Lambert, and Mittenhof, had been seen, which the jury of the exhibition of 1806 recognized as a true stone-ware capable of standing the fire, that is to say common porcelain*.

Stone-ware may be made to stand the fire.

Lauraguais.

Use of magnesia and barytes.

Artificial feldtspar.

Pumice stone as a glaze.

Hygiocérames.

Devitrification by heat.

Thus

* I have here pointed out only the principal facts. I might quote many others, that tend not less powerfully to confirm the opinion For instance, the spuma maris, the keffekil of Kirwan, to which the name of magnesite has been given, and of which the Turks make their pipes, contains according to Klaproth but 0 50 silex, and 0 17 magnesia I have found, that it loses 0·23 of its weight in the fire. It has the property of stopping both the vitrification and the contraction of the compositions into which it enters.

Keffekil, its use.

Mr. Giobert has shown, in the Memoirs of the Academy of Turin for 1802, that the earth of Baudissero, long considered as almost pure alumine, and used with success in the porcelain manufactory of Vinovo, is a magnesian earth, containing about 0·14 of silex

Magnesian earth.

Among the results of the synthetical essays made in my laboratory at the Imperial Polytechnic School, I obtained a glass perfectly similar to that

Glass from ar-

Every necessary kind of pottery may be made without any metal.

Thus in the present state of our knowledge it is far from impossible, that an artist perfectly acquainted with it, and improved by practice, should succeed in fabricating, as Mr. Jousselin professes, three kinds of pottery, to supply the place, 1st, of close grained stone-ware, for containing liquids and other matters, with or without glazing: 2dly, of less close grained stone-ware, with a brown glaze externally, and a white enamel internally, for culinary utensils: and 3dly, of delfts and white earthen-wares, retaining both elegance of form and lustre of glazing, without employing any metal.

Very cheap glaze of earths alone.

The enamel of which Mr. Jousselin announces the discovery is entirely earthy, and composed of materials so cheap, that the enamel, which now costs the manufacturer at the rate of 320 franks for a certain quantity of ware, will come to no more than 15 or 20.

X.

Process for proving the Quality of a Glaze of Earthen-Ware.*

Defects of glaze.

THE glaze of earthen-ware may have several defects: it may be scratched more or less readily by a hard body; weak acids, such as vinegar, lemon-juice, verjuice, &c., may attack and dissolve the lead it contains; or oily substances standing long on it may produce the same effect, stain it, and render it dull.

tificial feldtspar.

Porcelain without kaolin.

that afforded me by the feldtspar of Baveno, by urging to fusion in a platina crucible a mixture of 62 parts silex, 16 alumine, 10 lime, and 12 potash. I have likewise made, without kaolin, a biscuit having the hardness, semitransparency, and grain of porcelain, by giving the proper degree of baking to a paste composed of 50 parts silex, 20 alumine, 24 magnesia, and 6 lime. I need not say, that it would be very easy to employ the same proportions of silex and alumine, by choosing a good clay, without being obliged to have recourse to the decomposition of alum for the earth.

* Sonnini's Bibliothèque Physico-économique, July, 1807, p. 43.

To



To determine its power of resisting friction, it may be rubbed with sand; and if this scratch it more readily than it does a glaze known to be good, we may be assured it is soft. If soft, sand will scratch it.

If vinegar be boiled for some hours in a vessel coated with a soft glaze, it will attack the glaze, and dissolve a portion of its lead, which will be precipitated from the vinegar on the addition of a few drops of sulphuric acid, commonly called oil of vitriol. Vinegar will dissolve its lead.

But a method more within every one's reach, and therefore deserving to be known, is, to let fall a drop of strong ink on a piece of earthen-ware, dry it before the fire, and then wash it. If the glaze be too soft, the ink will leave on it a slight spot. Ink a ready test.

XI.

Heights of various Places in France, &c.; by Dr. BERGER. *Continued from p.* 217.

SECT. II.

Heights ascertained during a tour in the ci-devant Province of Auvergne.

THE following observations were collected in a tour made in the spring of 1802, in company with Mr. Leopold von Buch, a celebrated Prussian mineralogist, and Mr. A. Jurine. With these gentlemen I set out from Geneva to visit the chain of the mountains Dôme and d'Or, traversing the ci-devant provinces of Bugey, Bresse, Lyonnois, and Forez, and returning through Dauphiny. As all this country, particularly the most interesting, which is for the greater part included in the circle forming the department of Puy-de-Dôme, has been carefully examined by several able mineralogists, I shall say little respecting its physical constitution. Most of the heights were calculated by Mr. von Buch from the simple formula of the difference of the logarithms of the numbers expressing the heights of the barometers at the Tour in Auvergne.

the two stations*: neither were they made with such strict attention to accuracy, as to be considered as absolutely determined.

It would be difficult to add any thing to what Mr. von Saussure says of the road from Geneva to Lyons in his Tours to the Alps: I shall only enlarge a little more than he has done on some places where pebbles or blocks of primitive rocks occur. Following more or less closely the course of the Rhone from Geneva, we meet with some at the villages of Confignon and petite Grave, where they rest on a bed of soft gritstone; at Chancy, where we found on the banks of the Rhone a granite with reddish feldtspar; and in the bed of the London, a small river that comes from mount Jura, and falls into the Rhone, where there are several pebbles of serpentine, including tolerably large garnets. But in a marshy bottom situate below the village of Pougny a large quantity of primitive compound rocks are seen, some with a base of diallage [smaragdite of Saussure] and jade, others of almost pure jade, or compact petrosilex. Not far from the loss of the Rhone, near the village of Vauchy, primitive pebbles are still perceptible; afterward toward Chatillon they become more rare; yet I have seen blocks of gneiss on this road, about a mile from the little lake of Syant or Sylant, which no doubt formerly made but one with that of Nantua, about 120 yards below it. Every thing leads us to believe, as Mr. Saussure remarks, that the latter extended much further to the south-west, covering the large flat meadows observed in that quarter, the soil of which is composed of rounded pebbles for the most part calcareous.

From Geneva along the course of the Rhone.

Lakes Syant and Nantua.

From this place to the extremity of the Jura, between Poncin and Pont-d'Ain, scarcely any primitive pebbles occur. There we begin to meet with pebbles of quartz in considerable quantity, and some blocks of gneiss in the en-

Extremity of the Jura.

* "Though the correction for temperature, with respect to the dilation of the air, is indispensable in measuring differences of level in the same country and at the same time; it is not quite certain, that it ought to be employed when we compare countries very distant from each other, and take the mean of a great number of observations." J. B. Biot's Physical Astronomy, vol. I, p. 145.

virons

virons of Priay; whence the road to Lyons is over plains covered with pebbles, frequently in such quantity as to prevent the land from being cultivated. The prevailing species are quartz, and hard quartzose gritstone. The pebbles of the Alps indeed frequently occur, as micaceous schist, schistose hornblende, and serpentines: yet when we traverse the bed of any torrent or river coming from the adjacent mountains, the calcareous stones always predominate. Road to Lyons.

Between lake Sylant and Chatillon, about two or three miles from this town, on the left bank of the little river is a tolerably fine spring, called Entrebilliet, the temperature of which on the 1st of April was 7·5° of Deluc [49° F.], while that of the open air was 6·5° [46·7°]. The height of the place above the sea, as found by the barometer, was 241 toises. At Varambon, near Pont-d'Ain, a spring rose out of the ground, the temperature of which was 9° [52·25°], and that of the open air 12° [59°]. The height of this place was about 140 toises. These two observations agree sufficiently with the law established empirically by Mr. Saussure, that the heat of the air decreases about 1° of Deluc for every hundred toises in height. Heat of springs, agreeing with Saussure's law.

From Chatillon to Nantua we found a prodigious quantity of box. All the country, except the summits of the mountains, which are crowned with firs, is covered with this shrub; and from the warmth of its local aspect, it grows to a considerable size, as in Campania and the East. Box.

From the environs of the loss of the Rhone we do not meet with any vines in the road, till we reach Cerdon*. At this place is a plantation, reaching from the top of the mountain, which is 403 feet higher than the lake of Geneva, to the bottom of the declivity on the high road, which is 192 feet lower than the lake. Vines.

If in proceeding from Lyons into Auvergne we travel directly westward, traversing the Lyonnois and Forez, we are constantly on the primitive soil. The chief base of the country of Limagne, that fine part of France, is well known Granitic country.

* The extent to which Mr. Arthur Young has availed himself of his accurate observations on the locality of certain cultivated plants, among which is the vine, is well known. I do not hesitate to pursue his views of vegetable physics, when opportunity offers.

to

to be granitic: the same kind of soil too, that we meet with in going down the Rhone from Lyons by the way of Vienne and Tournon, occurs when we proceed to Beaujolois by way of Tarare, Thizi, and la Claire: thus from the considerable extent occupied by this kind of soil in these countries the centre of all these chains cannot be far distant, and it is not without reason, that Mr. Delametherie places it in the Cevennes, which he considers as one of the principal centres of the primitive mountains of France.

The Cevennes.

Mountain of St. Bonnet.

The rock that composes the mountain of St. Bonnet-le-Froid is an undulated reddish gneiss, evidently stratified, and intersected by strata or veins of other rocks, particularly white quartz, and schistose hornblende, which assumes a porphyritic appearance on the back of the mountain to the west, particularly between the villages of Coursieux and Ste Foy-l'Argentière. Throughout the whole district of the latter, situate in a valley watered by the little river Brévenne, pitcoal is found. Among the fruit-trees surrounding the houses a few chesnuts are to be seen, but all of them poor and low: probably this tree will not thrive without shelter.

Coal.

Chesnuts.

Vines and walnuts congenial.

On the back of the mountain of St. Bonnet, toward Coursieux, there are some fine plantations of vines; and in the bottom of the valley very fine walnut trees. This is not the only place where I have observed the vine and the walnut thriving together; probably therefore they require nearly a similar temperature.

Holly tall and smooth leaved at top.

The holly is common among the firs in the mountains of the Lyonnois, and sometimes appears as a tree ten or eleven feet high. In these cases the upper leaves are smooth, while the lower ones are prickly as usual.

Lake diminished to ponds.

Basaltic mountain.

The plain of Forez is covered with a multitude of ponds, and no doubt formerly was one extensive lake. In it there is a basaltic mountain. The level of this plain is 185 feet below that of Limagne. Its soil is evidently formed by the decomposition of the primitive rocks; but it appears perfectly adapted to wheat. One weed only infests all the fields, the wood sorrel; but this is in great abundance, particularly on the fallows: however it is beneficial to the sheep folded

Wood sorrel.

on



on them, for they prefer it to any other plant, whence Tabernæmontanus called it oxalis ovina. Sheep fond of it.

In the neighbourhood of Feurs I saw furze for the first time on my road, a plant not to be met with in any part of Switzerland. Furze.

Table of heights above the sea in toises and thousandth parts. Table of heights above the level of the sea.

Places.	Without cor for temperature.	According to Deluc.	According to Trembley.	As given by Deluc.
Chataillon de Michaille		286·068	287·945	264
Lake Syant		302·582	304·843	
Cerdon		191·820	193·874	158
Meximeux		131·080	132·634	118
Lyons	89·680			88
			By Saussure	84 or 85
Petit-St.-Jean	121·500			
The highest part of the road over St. Bonnet-le-Froid	390·500			
Coursieux	188·000			
Ste Foy-l'Argentière	229·500			
St. Martin de l'Estra	297·000			
Feurs	173·000			
St. Germain-le-Val	210·000			
St. Just-en-Chevalet	352·636			
The highest point of the chain of Thiers, taken on the road, near Arconsat		481·601	488·096	
Boen	200·000			
Noire-Table	344·500			
Thiers, at the lower part of the town	192·000			
Clermont	200·000			
Summit of the Pradelle, an ancient basaltic promontory, resting on granite, but separated from it by a thin stratum of bolar earth	352·500			

Orgines

Table of heights above the level of the sea.

	Without cor. for temperature.
Orgines *	393·000
The summit of Puy-de-Dôme †	751·657
Puy-de-Parion, on the edge of the crater	610·833
——— at the bottom of the crater ‡	574·166
Puy-de-Barme	561·166
Orcival	448·000
Mount Jughat, on the edge of the crater	574·166
——— at the bottom of the crater	552·500
Issue of the current of lava from Puy-de-la-Vache	509·000
Lake Aidat	419·000
Mountains of Croix-Morand	693·166
Village of Mont-d'Or-les-Bains	523·333
Cascade of la Dogne, at bottom	654·000
——— at the top §	694·333
Rock of the Cousins \|\|	885·333
Mountain of Cacadogne \|\|	902·666
	Summit

Lava.

* Here is found the grand stream of lava from Puy de-Pariou. This lava is very fragile, and contains only small crystals of feldtspar, which retain their *native lustre*.

Porphyry.

† This mountain is formed of a sort of porphyry, the cement of which, not very hard, and of an earthy gray colour, includes mica and a great many large crystals of feldtspar, which are cracked and have a vitreous appearance; while those found in the granite, that constitutes the base of these mountains, have a pearly gloss.

According to Perrier the height of this mountain above the sea is 730 toises; according to Cassini and le Monnier, 757.

Crater.

‡ This crater, which is perfectly circular, measures 200 paces round: it is covered with grass, and cattle feed in it.

Volcanic productions.

§ In this excursion fine feldtspars are found, in large double crystals, in a porphyry, which according to Mr. von Buch cannot have a very different origin from that of Puy-de-Dôme. He adds, that it is a volcanic production, but not lava.

\|\| If we proceed along the ridge of the mountain from the top of the cascade of la Dogne, says Mr. von Buch, we arrive at the rock of the Cousins, where we see substances that have much more appearance of

Basaltes.

basaltes, and in which the feldtspar becomes more rare. Continuing along the ridge toward Cacadogne, we go round a frightful, semicircular precipice, the sides of which are covered with scoriæ. This is the only

Crater.

place in the neighbourhood, that can be termed a crater. From Cacadogne the ascent to the summit of Mont-d'Or is easy. It is an immense

circus,



	Without cor. for temperature.	
Summit of Mont d'Or*	958·500	Table of heights above the level of the sea.
Circus at the foot of Mont d'Or, before the junction of the waters of the d'Or and the Dogne	611·000	
Summit of Mount Capuein†	709·500	
La-Tour-d'Auvergne‡	472·833	
Murat-le-Quayre, at the château	539·000	
Rocks on the Dordogne, about half a mile lower§	417·666	
St. Laurent-des-Mûres	117·000	
Bourgoin	173·200	
La-Tour-du-Pin	158·200	
Pont-de-Beauvoisin	118·000	
The lake of Epin or Aiguibellette	193·334	
Mount de l'Epin	463·666	

Sect. III.

Brief description of several mountains in the department of the Leman lake.

Mountains near the lake of Geneva.

Mount *Salève*, four miles east of Geneva, is narrow, but of considerable length from N. N. E. to S. S. W. On the W. N. W. it exhibits naked and steep rocks, in nearly hori- Salève.

circus, truly alpine, terminated on one hand by the sides of the valley of la Cour, and on the other by the rock of the Cousins. These answer to each other, and formerly closed the circus on the side of the valley of Bains. But the whole of this vast circus cannot be a crater. I conceive, continues Mr. von Buch, that there are two, the valley of la Cour, and the funnelshaped hollow between Cacadogne and the rock of the Cousins; the remainder of the cavity was formed by the falling in of the parts between these two craters, as is shown by the bare and salient angles below the summit of Mont d'Or, and the valley of Enfer. Formerly two.

* According to Cassini the height of this mountain above the sea is 1048 toises: and he afterward calculated by the barometer its height above the village of les Bains to be 512 toises, while according to our observations it is only 4 :5.

† Cassini makes this 224 toises higher than les Bains, and 760 above the level of the sea.

‡ A causeway of basaltes in prisms of six feet diameter, with decisive appearances of having been originally a stream of lava, is seen here. *Von Buch.* Basaltes evidently from a volcano.

§ In this place is a grand colonnade of basaltes resembling the pipes of an organ. Colonnade of it.

zontal

Curved strata under-horizontal.

that surround the valley from Samoin are singularly distorted in several places. Among others I observed at the bottom of the mountain called Pointe de Sale strata bent concentrically on one another, so as to form a large ellipsis, and over these horizontal strata. This is one instance of many confirming Mr. Saussure's remark of the irregularity of the strata of secondary mountains as they approach the primary.

Goitres.

I did not find a single block of primitive stone in this valley. At Samoin, the aspect of which is certainly the hottest in the valley, I observed some *cretins*. On the left of the road from Taninge, about an hour's journey before we reach Samoin, is a chalybeate spring.

Chalybeate spring.

Brison or Brézon.

Mount *Brison*, called *Brézon* by naturalists and geographers, forms with the Môle the entrance of the vale of Arve. It is of compact limestone, and W. S. W. from the Môle. Its summit toward this mountain is perpendicular to a great depth, while its strata slope toward the Alps, though very steep at the top. Its foot is covered with large strata, nearly perpendicular, resting against the body of the mountain. In it there is a natural icehouse, containing ice all the year round, though it is of little depth. On the 25th of July, 1800, the temperature of this cavern was 0° [32° F.] while that of the open air was 9·5° [54·5°].

Natural ice-house.

Vergi.

The name of *Vergi* is given to a chain of loftier calcareous mountains behind this. It declines to the north-east, and rises to the south-west, without any remarkable and distinct summit. On its back are two lakes; that of Saxonnex, or Béni, to the north; and Lessy, which is larger but not so deep, to the south. This has no apparent outlet; but between the little town of Entremont and that of petit Bornand there is a pretty considerable cascade, spouting forcibly from a circular aperture in the middle of a rock fifty or sixty yards below the summit of the mountain, and fed by the lake. The limestone that forms this chain is of a dirty gray, of a foliated texture like the schists, and in general rising against the Alps at an angle of 31° or 33°. This is not uniform however, for to the north-east above Scionzier the strata are arched, or bent double, and of a compact instead of a slaty texture. All this chain, which some geographers still call *les montagnes maudites*, is very rich in

Lakes Saxonnex and Lessy.

Arched strata.

plants,

plants, some of them rare. On the ground accumulated at the foot of the Encrenaz, above lake Saxonnex, grows the alpine poppy, remarkable for its beautiful milk white petals, and its agreeable smell of vanello, particularly when the flower first opens. Alpine poppy.

On the east of this chain is the valley of *le Reposoir*, which on the opposite side is bounded by another calcareous chain of very lofty mountains. A convent of Carthusians here is an excellent station for a naturalist, who would wish to explore the neighbouring mountains, which are in many respects interesting. Having ascended mount Meiri to Point-de-Château, one of the loftiest summits, the eye takes in the whole chain of the Alps, and looks down upon the vale of the Arve beneath. The highest point however is a little farther west, and is called the mountain du Four, or de Pierre percée, because its ridge is perforated. This mountain is seen from every part of the Alps, and at a distance appears inaccessible, though it is not so. Had it been later in the year I would have ascended it, but I was there in July. Vale of le Reposoir. Mount Meiri.

The road from St. Martin to Servoz passes at the foot of a chain of mountains, the base of which is composed of slate, or a brown calcareous stone in thin leaves, intersected by veins of calcareous spar or quartz. You then ascend to the pleasing lake Chède; soon reach the fragments of a mountain, the summit of which fell down in 1751; cross le Nant-Noir; and, having passed a forest, the soil of which is a yellowish tufa, reach Servoz, where we meet with a few persons afflicted with the bronchocele, owing probably to its southern aspect, and being sheltered from the north winds. Lake Chède. Fragments of a fallen mountain. Cretins.

Ascending toward the point called l'Aiguille de Varens, we meet with petrifactions in a transition limestone about 1200 toises above the level of the sea. It has no impressions of vegetables, but a large species of turbinite [*vis*], and a bivalve of which frequently nothing but the edges remain. The real summit of the mountain is higher than this peak, but cannot be seen from the bridge of Sallenches, being behind the peak, which, as well as many others, is separated from the mountain by a kind of circus of considerable depth. On the summit of Varens is an extensive bed of numismal stones, and some hundred toises below, among the mutters Peak of Varens. Petrifactions.

that have fallen from the mountain are many remarkably large cornua ammonis.

Buet. Different roads up it.

Mount Buet may be ascended not only from the valley of Sixt, taken by its first visitors, Messrs. Deluc; or that of Bérard, the way chosen by Mr. Bourrit, and described by Mr. Saussure; but by crossing mount Bréven from the priory of Chamouni, or from Servoz, by going up the valley of Villy. This valley is watered by a rivulet, which forms the precise boundary between the primary and secondary rocks. Another observation to be made in this road is, that the mountain above Servoz, which terminates the chain of Aiguilles rouges to the south-west is in great part formed of a primitive micaceous gritstone; there being only the summit called Aiguilette, that is composed of a foliated rock like that of mount Bréven. In like manner at the north-east of the chain the pass of Charlenton is formed of a primitive gritstone, remarkable for its well defined small quartz crystals; and is the intermediate point between the primary rocks of the chain of Aiguilles rouges and the transition limestone of the summit of Buet. This is another instance in support of Mr. Saussure's remark, that beds of grit or puddingstone are almost always found between the last secondary and first primary strata.

Vale of Villy.

Grit or puddingstone generally between the primary and secondary strata.

Valley of Chamouni. Contains some secondary strata.

The valley of Chamouni runs south-west and north-east, parallel to the chain of the Alps, and is bordered by mountains of the primary class. The col de Balme however, that bounds it on the north-east, and mount Lacha, its termination at the south-west, are composed of slate or calcareous stones. This valley contains other secondary rocks likewise, as some fine white gypsum, about three miles south-west of the priory, on the borders of the *nant* or torrent of Taconay; some limestone opposite the priory, at a place called Biolay; and the isolated hill of Piget, in the bottom of the valley, and stretching in the same direction, which is entirely formed of limestone. These excepted, every thing in the valley of Chamouni is of the primitive class.

A calcareous hill.

Bréven.

Exactly above the priory, on the north-west of the valley, is mount Bréven, the base of which is connected with the Aiguilles rouges. Its summit is isolated, and its strata are cut off perpendicularly on this side, but slope gently on the other

other toward the valley of Villy, which is parallel with this. Its ascent from the priory, though steep, is practicable to about two thirds of its height, through fragments of lamellar rocks, mixed with quartz, feldtspar, and mica, in every possible proportion, and of different degrees of hardness, from the hardest lamellar granite to the softest micaceous rock. These fragments come either from the summit or from the flanks of the mountain. The summit in particular is formed of a rock, that appears to belong to the class of true granites, notwithstanding the parallel situation affected by the scales of mica, that form part of it. The back of the mountain is composed in great measure of a veined granite, with lenticular crystals of quartz of various sizes, but ranging in the direction of the laminæ.

The Aiguilles, or needles, on the south-east of the valley of Chamouni, are five lofty pyramids, decreasing in height from the southernmost to the northernmost. The base that supports them, which rises seven or eight hundred toises above the valley, is composed of lamellar rocks of different kinds, but chiefly quartzose or micaceous, arranged in very regular strata, and running in the direction of the valley. Their inclination toward the bottom of the mountains is very little, but they gradually rise against the valley to the top, where they are exactly vertical. The higher up the mountain they are, the more they approach the nature of granite. The pyramids that rise above this are of granite in mass. They are composed externally of pyramidal laminæ, subdivided into strata parallel to the planes of the laminæ themselves. These laminæ are nearly vertical, and do not lean against the valley, like the lower strata of the mountain, but against the body of the pyramid. The heart of the pyramids, or their interior part, appears in some places to have no regular structure, and to be divided only by accidental clefts. These pyramids however must not be imagined to stand upon the mass below, as a pillar on its base: on the contrary they must have a base of their own beneath, on which the strata of this mass in part rest. Needles.

The Aiguille du Dru is of a nearly similar structure, but among the fragments at its foot are blocks of primitive petrosilex, with a great deal of feldstpar, and a little mica. Aiguille du Dru.

Mount Jura. The Jura is a chain of calcareous mountains 150 or 200 miles long, by 35 or 40 broad: running S. S. W. and N.N.E. from the neighbourhood of Pouçin in Bresse to Basil. Its course therefore is nearly parallel with that of the Alps, of the external chains of which it must be considered as a dependance. It is not very easy to mark the situation and form of the strata of the Jura. Mr. Saussure thinks, that the strata of the eastern chain, which is the loftiest and the nearest the Alps, rise leaning against the centre of the chain, and decline on the opposite side; while the strata of the following chains to the west have the form of arches, or semi-arches, and terminate in plains, the base of which consists of horizontal beds of limestone.

Dôle not so high as Reculet. The Dôle, twelve miles north of Geneva, has been generally deemed the highest summit of the Jura. According to Mr. Deluc it is 658 toises above the lake, and consequently 847 above the sea. I consider the Reculet, ten miles north-west of Geneva, as rather higher. It is one of the number of mountains in the chain, that appear to contradict the general form of the strata above given; for, instead of rising against the centre of the chain, they present their cliffs to the Alps.

Reculet. In an excursion I made on the Reculet, the 7th of August, 1802, I ascertained the temperature of two springs at the same time with their height above the sea. The temperature of one, situate above the *chalet* of Arderan, and 730 toises above the sea, was 4·8° [43° F.], while that of the open air was 21·5° [80·3 F.]. That of the other, called Converse, 815 toises above the sea, was 4° [41° F.], the thermometer in the open air being at 20·5° [78° F.]. These two observations agree sufficiently with the law of the decrease of heat laid down by Mr. Saussure; the following is not quite so consistent with it. On the 29th of August, 1802, the thermometer in a fine spring near the village of Veiri, at the foot of mount Salève, 200 toises above the sea, stood at 8·5° [51·2° F.], and in the open air at 13·7° [62·8° F.].

Temperature of springs.

Agrees with Saussure's law.

Another does not.

XII.

Transformation of Mr. Dubuat's Hydraulic Theorem. By THOMAS YOUNG, *M.D. F.R.S.*

To Mr. NICHOLSON.

SIR,

IN the course of some investigations respecting the motion of the blood, and the cause of fever, I have had occasion to employ the rules derived from the hydraulic experiments of Dubuat; and having reduced them into a more correct form than those which are mentioned in the second volume of my Lectures on Natural Philosophy, p. 225, I beg leave to make public, through the medium of your Journal, the formula which I have obtained. Dubuat's hydraulic theorem transformed.

Dubuat's rule, reduced to English inches, is $\frac{l + 45d}{h} = b$,

and $v = 153\ (\sqrt{d} - \cdot 2)\ .\ (\frac{1}{\sqrt{b} - \text{h.l.}\sqrt{(b + 1 \cdot 6)}} - \cdot 001)$

l being the length of a pipe, d its diameter, h the height of the reservoir, and v the velocity in a second. Now $\text{h.l.}\ x = n\, x^{\frac{1}{n}} - n$, when n is infinite, for the fluxion $\frac{x^{\frac{1}{n}}\dot{x}}{x}$ becomes ultimately $\frac{\dot{x}}{x}$; and the same is very nearly true when n is any large number: we may therefore express the hyperbolical logarithm, taking $n = 16$, in the terms $8\, b^{\frac{1}{8}} - 8 + \frac{8}{b}$; and the whole formula may be changed into another, which will often be more convenient, in this manner, making $c = b^{\frac{1}{8}} - 1$, $v = 153\ (\sqrt{d} - \cdot 2)\ .\ (\frac{10\sqrt{b} + 75c + 8}{10\, b}$ $- \cdot 0012.)$ This expression will be found as near the truth, in all cases, as can be supposed to have been correctly ascertained by Dubuat's experiments.

I am, SIR,

Your very obedient servant,

THOMAS YOUNG.

Welbeck Street, 22 Nov.

XIII.

XIII.

Observations on the Theory of Ear Trumpets, with a View to their Improvement; by JOHN GOUGH, *Esq.*

SIR, *Middlesbaw, 20th Nov.* 1807.

An improved ear trumpet requested.

ONE of your correspondents, who appears at page 51 of your XIIIth volume, under the signature of A. B. has lately addressed me in the same anonymous character on the subject of ear trumpets. He requests to learn my sentiments respecting these instruments, and hints leading to their improvement through the medium of your Journal.

The subject hitherto neglected by philosophers.

I believe but little attention has been hitherto bestowed on this part of acoustics, though the inquiry is intimately connected with the ease and happiness of the partially deaf of all ranks and ages. This negligence in experimental philophers, who have done so much for the improvement of optics, obliges me to begin with a fundamental and essential point of my subject; for we are in uncertainty at present in what manner an ear trumpet acts on the auditory organs. We may conceive the sonoriferous pulses to be collected in the cavity of the vessel, and to pass thence into the meatus auditorius in a state of increased condensation.

Two theories of the ear trumpet proposed.

On the other hand, we may suppose, that the same pulses strike the sides of the trumpet, and excite similar vibrations in this metallic shell, which imparts them to the orifice of the auditory duct. The latter supposition is rendered plausible by a simple experiment; if the porches of the ears be securely stuffed with wet paper, you may convey the clicking of a watch along a rod of wood or metal to the seat of hearing by simply touching the watch with one end of the rod, and pressing the other against the forehead, your teeth, or the wet paper in your ears. The preceding fact, in conjunction with other circumstances and arguments, induced me at first to prefer the second theory of ear trumpets, and to conclude, that the vibrations of the metal constitute the real cause of augmented sound rather than the condensation of sonoriferous pulses.

The latter theory plausible.

After

After taking this view of the subject, I was inclined to adopt your opinion in vol. XIII, page 52, that an instrument consisting of a broad thin surface, furnished with a tail or stem, promises to relieve partial deafness as effectually and more conveniently than a trumpet. A number of experiments, however, made for the purpose, have convinced me, that the vibration excited in thin plates of metal, wood and pasteboard, by sonoriferous pulses of air, cannot be condensed in a stem attached to these substances, so as to be conveyed with effect to the seat of hearing. My first trial was conducted in the following manner: a hole was made through a partition of lath and plaster, which was just large enough to receive a rod of deal 2 feet long, and $\frac{1}{4}$ an inch in diameter. Some circular plates of metal were provided at the same time, as well as thin boards and pieces of pasteboard of the same figure, which were fixed at pleasure on the ends of the rod, by means of holes in their centres. This contrivance gives the observer an opportunity of placing himself in one room and the sonorous body in another; and this precaution prevents all the pulses from reaching his ear, except those that are conveyed by the rod, provided the force of the sound be too weak to make its way through the partition itself. The effects produced by this apparatus were the following: when one of the circular plates properly mounted on the end, was slightly scratched with a pin, or even with a piece of twisted paper, the sound passed very distinctly along the deal into the room where the observer was situated, and was thrown off into the air from a circle of wood or metal fixed on the rod in that apartment. The same circumstance took place when a watch was brought into contact with one of the circles, and the observer stood near the other; but absolute contact was found to be necessary, for the sound ceased as oft as the watch was removed the shortest distance from the circle.

Mr. Nicholson's idea, vo XIII, page 5 examined.

Not continue by experiment.

The discovery of this fact damped the expectation which I had hitherto entertained of affording relief to partial deafness by solid conductors of sound, but not to dismiss the inquiry apparently in a negligent manner, I procured two or three plates of different elastic substances, furnished with slender tails

Other experiments to the same purpos

tales of wood, with which I made an unsuccessful experiment on the ears of a lady who labours under a considerable degree of nervous deafness. An attempt was also made to convey weak sounds by the same instrument to the auditory organs through the medium of the teeth, when the ears were stopped; but all these trials failed, unless the sonorous body happened to touch some part of the apparatus.

This succession of disappointments convinced me, that solid conductors can be of no advantage to the partially deaf.

Probable use of solid conductors.

Instances of great insensibility may occur indeed, in which we may arrive at the seat of hearing by their assistance, through the channel of the mouth, after every trial to approach it by the natural ducts and passages have proved fruitless. In this manner, perhaps, some persons apparently in a state of absolute deafness, might acquire some idea of the musical scale by attaching one end of a stick to a harpsichord, and holding the other in the mouth. At the same time, I am apprehensive your correspondent A. B. will be unfortunately disappointed in his expectations of relief from conductors, which are to be held in the teeth in the manner of a tobacco pipe.

An experiment on the ear trumpet.

The next experiment relates more directly to ear trumpets, and discovers the mode in which they operate on the auditory organs. I took a hollow copper cone, the mouth of which was 6 inches in diameter, and having closed one of my ears with wet paper, I introduced the small end of the tube into the other, taking care to cover that side of my face with a folded handkerchief, with a view to intercept as much as possible such vagrant pulses as were not received by the trumpet. Upon directing the wide end of my clumsy instrument towards a watch on a table, I found that it magnified the strokes of the balance beyond my expectation. But this was the case only while the tube remained open, for the watch ceased to be audible after a plug of wet paper had been forced into the narrow part of the tube, at the distance of 2 or 3 inches from the smaller extremity. This experiment points out the office of an ear trumpet in a satisfactory manner. Its business is to condense the pulses which happen to fall into its cavity, and thereby to discharge them with

Trumpets condense sonoriferous pulses.

greater

greater effect into the auditory ducts; our attention therefore, must be turned in future to the most probable means of increasing the condensing power of the instrument, if we wish to mitigate the inconvenience of nervous deafness.

Parabolic trumpets considered.

A vessel of a parabolical figure, and well polished, promises to be of service to the infirmity of partial deafness, on a superficial consideration of the subject, because such an instrument would concentrate in its focus all the pulses which happened to enter its mouth at the same instant, in a direction parallel to its axis. A trumpet of this description is liable to two serious objections, for the construction of it is very difficult, if not impossible; and if such a thing could be made, it would be attended with inconveniences, arising from its shape and dimensions, that would render the application of it very troublesome. This may be easily proved by a few simple calculations derived from the properties of the parabola; in reality it may be feared, that the partially deaf have little to expect from any kind of ear trumpets, but those of a conical figure; and apparently strong reasons lead me to suspect the best of them to be very imperfect augmenters of sound. Perhaps I may take a future opportunity to consider their defects mathematically; but it will be sufficient for the present purpose to observe, that very few of the pulses received at the mouth of a conical tube are transmitted to the ear through the opposite extremity. This might be demonstrated on the well known laws of incidence and reflection, and the truth of it is experimentally proved by the office of the funnel in the invisible lady, which does not transmit the whisper committed to it so much as it reflects it.

The improvements of conical trumpets difficult.

An instrument on the principle of a drum recommended.

It may be here naturally asked, if the partially deaf are to lay aside all future hopes of additional relief from the improvement of acoustic instruments? I will not venture to give a decisive answer to this question; perhaps future experiments may discover a more convenient and efficacious form of the ear trumpet than any in use at present; but I would recommend your correspondent A. B. or some one of his fellow sufferers, to exchange his trumpet for a drum, by way of trial. Perhaps this hint will appear obscure in its present form,

form, and an explanation will be expected. By a drum I mean a circular box, or funnel, furnished with an ear pipe, and having its mouth or widest aperture covered with a thin elastic membrane, which must be stretched with an uniform force in every direction, like the vellum of a military drum. The pulses which fall from the atmosphere upon this membrane will be immediately transmitted by it to the air confined in the box, and their escape from this cavity must evidently be through the ear tube, because the covering of the mouth will not permit them to return into the atmosphere by that aperture.

An experiment made with such an instrument.

The instrument here recommended is not a mere project of theory, for I have made some experiments, which induce me to hope for beneficial consequences from a contrivance of the kind. I took a metal funnel of 2 inches radius, and a circular wood box of the same diameter, which was a segment of a sphere of 8 inches radius. The mouth of each vessel was covered with a piece of hog's bladder, moistened in water, and securely fixed by a thread to the outside of the instrument. The bladder contracted in drying, by which it acquired a considerable degree of tension; and I do not hesitate to say, that both these drums, when properly applied to my ear, augmented the beats of a watch and other weak sounds, in a manner which was very perceptible. Should the experiment, however, appear worthy of further attention, it must be left in future to those who expect advantage from it, because such inquiries are attended with the greatest difficulties, when undertaken by persons who possess the sense of hearing in perfection. If your correspondent A. B. should conclude to pursue the subject, he will recollect, that a fine membrane is prepared from the intestines of cattle, which is called gold beater's skin, at least in the north of England; and it is unnecessary to inform him, that this substance is preferable in every point of view to the bladder which I used in my experiment.

The further prosecution of the experiment left to the partially deaf.

JOHN GOUGH.

P. S. I have long expected, that some of your correspondents would call an opinion in question, which I advanced nearly

nearly two years ago in the Philosophical Journal; but their silence obliges me to become my own accuser. Some experiments of mine will be found in your number for March, 1806, from which I inferred, that water cannot retain its fluidity when cooled below 32°. Such was my opinion at the time, but I was soon induced to suspect its accuracy from a conversation on the subject with Mr. Dalton, of Manchester. The glass vessels used in my experiments were externally covered with the freezing mixture nearly to their brims; but in consequence of this gentleman's information, I repeated the same experiment last winter, care being taken to raise the upper half of the vessel, containing the water, above the salt and snow which surrounded its bottom. With this precaution I found water might easily be cooled many degrees below the freezing point; in consequence of which discovery, I was obliged to abandon the theory referred to above, and in pronouncing it to be an errour I am only doing justice to the truth and your excellent miscellany.

Water may be cooled below 32° C

XIV.

Report made to the Mathematical and Physical Class of the Institute, on a Memoir of Mr. DESCOTILS, *relative to Iron Spar: by Messrs.* BERTHOLLET, LELIEVRE, *and* VAUQUELIN*.

IN January 1806 Mr. Descotils read to the class a memoir in which he proved by experiments, that the iron spar, which was the subject of it, varied in the proportions of its constituent principles; and hence he explained the differences that the ores require in their metallurgic treatment. The difficulty of fusing some of them constituted at that time the principal object of his research; and the comparative analysis he made led him to the conclusion, that the magnesia, which is frequently found in them in large quantity, was the cause of their refractoriness.

Iron spars vary in their principles, & should be differently treated.

Their refractoriness owing to magnesia.

Reflecting on the processes adopted to deprive these ores

Processes by

* Annales de Chimie, vol. LXII, p. 135, May, 1807.

of

which they are deprived of it.

of the principle of their infusibility, which consist chiefly in exposure to the air and rain, either before or after roasting, Mr. Descotils conjectured, that these processes had no other effect than that of separating the magnesia.

Their mode of operation.

In the first case, that is to say, when these ores were exposed to the air before roasting, he supposed, that this earth was dissolved in the state of carbonate by the rain. In the second, on the contrary, he ascribed this effect to the sulphuric acid developed by the efflorescence of the pyrites, with which the iron spar is almost always accompanied.

Since that period Mr. Descotils has communicated to this assembly a second memoir, in which he furnishes substantial proofs of the explanations he had offered in the former paper as merely conjectural; at the same time avails himself of them to answer some objections, that had been advanced by Mr. Hassenfratz. The latter gentleman however, after having made some fresh experiments and observations, has withdrawn his memoir, which the class had referred to the same committee: we shall not therefore enter into any discussion of the points, on which these two learned chemists differed, but shall consider the facts related by Mr. Descotils, and the conclusion he has deduced from them, as if they had never been disputed.

Hassenfratz started some objections, but appears to have given them up.

The author has repeated his experiments, and made fresh.

On this second occasion Mr. Descotils has repeated his former experiments, which gave him the same results. He has likewise made new ones; and all, mutually supporting each other, have only confirmed him in his opinion. But let us relate some of these experiments.

Experiment with ore of Elba.

He exposed to the fire a mixture of fifteen parts of magnesia, and a hundred parts of iron ore from the isle of Elba, finely powdered; and the result he obtained was perfectly similar to what every magnesian iron spar has furnished him.

Effect not owing to its being powdered.

To ascertain whether the division of the particles of the substance had any influence on its fusibility, he made a trial with part of the same specimen of iron ore of Elba, without wasting or powdering it, and he obtained a perfectly compact button, at a degree of heat similar to what would have been requisite for an assay of earthy iron ore with the addition of borax.

Refractoriness

This fact shews, says the author, that cohesion does not diminish



diminish the fusibility of iron ores; at least if this cohesion can be estimated by the hardness of the ore, and the resistance it offers to the action of acids, for none possess these two qualities in a more striking degree than the iron crystals of the isle of Elba. The committee are of a similar opinion, only the fusion must require so much longer time in proportion as the ore is in fragments of a larger bulk. Therefore not owing to cohesion.

Mr. Descotils could have wished to analyse specimens of refractory iron spar comparatively with specimens of the same ore become fusible by exposure to the air: but not having been able to procure any, he thought he might supply their place by two pieces from the same vein, one of which was not altered, the other had passed to the state of a free ore.

Without describing the method he employed for this purpose, which we consider as very accurate, we shall only say, that he found the decomposed ore no longer contained any magnesia or carbonic acid, while the other contained four per cent of carbonic acid and magnesia. But to magnesia.

The analysis of five other specimens of free ores, from different places, gave him the same results, whence he concludes, that the separation of the magnesia is complete when the decomposition of the ores is complete. Farther proofs.

In some cases he suspects, that it is to the efflorescence of the pyrites, from which scarcely any sparry iron ore is free, that the solution and abstraction of the magnesia of the raw ore is owing; since sulphate of magnesia is sometimes to be observed on heaps of ore of an analogous nature exposed to the air, as well as in the waters with which these ores are washed; and he has obtained similar results in a small way, by putting magnesian iron spar in powder, into a solution of sulphate of iron. Mode in which the magnesia is removed.

He believes however, that it is most frequently the carbonic acid, which, disengaged from the iron in proportion as this absorbs oxigen, dissolves and carries off the magnesia by means of water. Action of carbonic acid.

As to the change effected in the roasted ore by exposure to air and rain, the conjectures of Mr. Descotils are confirmed by analysing the waters, with which a heap of roasted ore long exposed to the air had been washed. These waters contained Effect of roasting.

contained nothing but sulphate of magnesia, and a little sulphate of lime; which salts could have been produced only by the action of the sulphuric acid, arising from the pyrites, on the earthy substances contained in the ore.

Old ores more fusible than new.

Mr. Descotils quotes letters of several well informed persons, and worthy of credit, who, in agreeing on the point that sparry iron ores recently extracted and roasted are more difficult of fusion, and less productive, than those that have remained three or four years in the open air, give still more force to his theory.

Magnesia most injurious to rich ores.

Though it is certain, that the presence of magnesia in iron ores diminishes their fusibility more or less, the author of the memoir observes however, that, if it be accompanied with a sufficient quantity of lime, silex, and alumine, or of oxide of manganese, it is not so injurious, because it becomes fusible by combining with these substances.

No external marks of its presence.

Conceiving the advantage iron masters would find in having an easy method of knowing by simple inspection a free from a refractory ore, Mr. Descotils has examined, whether among the external characteristics of these substances there might not be some, by which these properties could be distinguished: but the strictest scrutiny in this respect was without success. He has been obliged therefore, to have recourse to chemical means, and what he found most to the purpose was fusing the ore without the addition of any flux.

Application of heat alone the best test.

Marks of a refractory ore.

If after this operation the matter present itself in a grayish, earthy, friable mass, interspersed with small globules of cast iron, it is a proof, that the ore is magnesian, and consequently more or less refractory.

Marks of a fusible ore.

But on the contrary, if a well fused button be obtained, with brown and not very abundant scoriæ, the ore is fusible, and contains but little magnesia.

Indication of manganese.

When the scoriæ are green, they indicate the presence of oxide of manganese, part of which is reduced, and mixes with the cast iron, by a high and long continued heat.

Loss in roasting.

The least altered kinds of sparry ores, that Mr. Descotils assayed, lost in roasting from 31 to 37 per cent. The altered or free ores lost at most but 14 per cent, and this loss was merely water.

Quantities of

The quantities of magnesia and manganese vary greatly: sometimes

sometimes there may be 12 per cent of either in the raw ore, and at others there is scarcely any. manganese and magnesia.

From the results of his analyses Mr. Descotils concludes, that a high proportion of one excludes a high proportion of the other, without the absence of the one necessarily indicating the presence of the other; so that the iron, when brought to the state of red oxide, always amounts to 50 per cent at least. Never a maximum of both.

Hence Mr. Descotils explains what takes place in the Catalonian forges, where the different species of ore are treated according to the nature, number, and quantity of the principles they contain. He points out the method, that each requires, and the product they afford, according as the operation is conducted. Sometimes it is cast steel, at others malleable iron, or some mixture of the two. On this occasion he expresses his surprise, that no one has yet thought of establishing a manufactory of cast steel in the Pyrenees. Catalonian works. Steel from the the Pyrenean ore.

He thinks justly, that all rich iron ores, which contain but few earthy parts, such as those of the island of Elba, might be fused with advantage in the Catalonian method. Rich ores.

It follows evidently from the experiments of Mr. Descotils, that certain kinds of sparry ores owe their infusibility to the presence of a large quantity of magnesia: and that the principal object of the exposure of these ores to the air and rain, either before or after roasting, is to separate the magnesia, and render them fusible. The various experiments we have witnessed, and the results of which we have seen, leave us no doubt on this head: since on the one hand the ores in which there is no magnesia are easy of fusion, and those which contain a certain proportion are wholly infusible; while on the other the addition of magnesia to fusible ores divests them of this property, and infusible ores, when their magnesia is abstracted from them, become fusible. General deductions.

From the observations of Mr. Descotils it farther follows, that there is no external character, by which we can distinguish whether a sparry iron ore be fusible or not: but he has pointed out chemical means of determining their nature, which are easy to put in practice.

Hence we are of opinion, that Mr. Descotils has thrown much light on the working of sparry iron ores; and that, as his

II. *On the Changes produced by Electricity in Water.*

Early observations of acid and alkali in Voltaic experiments.

The appearance of acid and alkaline matter in water acted on by a current of electricity, at the opposite electrified metallic surfaces, was observed in the first chemical experiments made with the column of Volta *.

Mr. Cruickshank † supposed, that the acid was the nitrous acid, and the alkali ammonia. M. Desormes ‡ soon after attempted to show by experiments, that muriatic acid and ammonia were the products, and M. Brugnatelli § asserted the formation of a new and peculiar substance, which he has thought proper to call the electric acid. The experiments said to be made in Italy, and in this country, on the production of muriate of soda are recent ||, and the discussions with regard to them still alive. As early as 1800, I had found that when separate portions of distilled water, filling two glass tubes connected by moist bladders, or any moist animal or vegetable substances, were submitted to the electrical action of the pile of Volta by means of gold wires, a nitro-muriatic solution of gold appeared in the tube containing the positive wire, or the wire transmitting the electricity, and a solution of soda in the opposite tube **; but I soon ascertained, that the muriatic acid owed its appearance to the animal or vegetable matters employed; for when the same fibres of cotton were made use of in successive experiments, and washed after every process in a weak solution of nitric acid, the water in the apparatus containing them, though acted on for a great length of time with a very strong power, at last produced no effect upon solution of nitrate of silver.

In cases when I had procured much soda, the glass at its

* Nicholson's Journal, 4to. Vol. IV, p. 183.

† Ibid. Vol. IV, p. 261.

‡ *Annales de Chimie*, Tom. XXXVII, p. 233.

§ Phil. Mag. Vol. IX, p. 181.

|| By M. Pacchioni, and by Mr. Peele. Phil. Mag. Vol. XXI, p. 279.

** I showed the results of the experiment to Dr. Beddoes at this time; and mentioned the circumstance to Sir James Hall, Mr. Clayfield, and other friends in 1801.

A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

SUPPLEMENT TO VOL. XVIII.

ARTICLE I.

On some Chemical Agencies of Electricity. By HUMPHRY DAVY, *Esq. F. R. S. M. R. I. A. Read November* 20, 1806 *.

1. *Introduction.*

Introductory remarks.

THE chemical effects produced by electricity have been for some time objects of philosophical attention; but the novelty of the phenomena, their want of analogy to known facts, and the apparent discordance of some of the results, have involved the inquiry in much obscurity.

An attempt to elucidate the subject will not, I hope, be considered by the Society as unfitted to the design of the Bakerian Lecture. I shall have to detail some minute (and I fear tedious) experiments; but they were absolutely essential to the investigation. I shall likewise, however, be able to offer some illustrations of appearances, which hitherto have not been fully explained, and to point out some new properties of one of the most powerful and general of material agents.

* From the Philosophical Transactions for 1807, Part I.

Changes produced by electricity in water.

strongly reddened. Paper coloured by turmeric introduced into the other tube had its colour much deepened; the acid matter gave a very slight degree of turbidness to solution of nitrate of silver. The fluid that affected turmeric retained this property after being strongly boiled; and it appeared more vivid as the quantity became reduced by evaporation; carbonate of ammonia was mixed with it, and the whole dried and exposed to a strong heat: a minute quantity of white matter remained, which, as far as my examination could go, had the properties of carbonate of soda. I compared it with similar minute portions of the pure carbonates of potash and soda. It was not so deliquescent as the former of these bodies, and it formed a salt with nitric acid, which like nitrate of soda soon attracted moisture from a damp atmosphere, and became fluid.

This result was unexpected, but it was far from convincing me, that the substances which I had obtained were *generated*. In a similar process with glass tubes, carried on exactly under the same circumstances, and for the same time, I obtained a quantity of alkali which must have been more than twenty times greater, but no traces of muriatic acid. There was much probability, that the agate might contain some minute portion of saline matter, not easily detected by chemical analysis, either in combination, or intimate adhesion in its pores. To determine this, I repeated the experiment a second, a third, and a fourth time. In the second experiment turbidness was still produced by solution of nitrate of silver in the tube containing the acid, but it was less distinct; in the third process it was barely perceptible: and in the fourth the two fluids remained perfectly clear after the mixture. The quantity of alkaline matter diminished in every operation; and in the last process, though the battery had been kept in great activity for three days, the fluid possessed in a very slight degree only the power of acting on paper tinged with turmeric; but its alkaline property was very sensible to litmus paper slightly reddened, which is a much more delicate test: and after evaporation and the process by carbonate of ammonia, a barely perceptible quantity of fixed alkali was still left. The acid matter in the other tube was abundant; its taste was sour; it smelt like water over which large quantities

tities of nitrous gas have been long kept; it did not affect solution of muriate of barytes; and a drop of it placed upon a polished plate of silver left after evaporation a black stain, precisely similar to that produced by extremely diluted nitrous acid. Changes produced by electricity in water.

After these results, I could no longer doubt that some saline matter existing in the agate tubes had been the source of the acid matter capable of precipitating nitrate of silver, and of much of the alkali. Four additional repetitions of the process, however, convinced me, that there was likewise some other cause for the presence of this last substance; for it continued to appear to the last, in quantities sufficiently distinguishable, and apparently equal in every case. I had used every precaution; I had included the tubes in glass vessels out of the reach of the circulating air; all the acting materials had been repeatedly washed with distilled water; and no part of them in contact with the fluid had been touched by the fingers.

The only substance which I could now conceive capable of furnishing the fixed alkali was the *water itself*. This water appeared pure by the tests of nitrate of silver and muriate of barytes; but potash and soda, as is well known, rise in small quantities in rapid distillations; and the New River water, which I made use of, contains animal and vegetable impurities, which it was easy to conceive might furnish neutral salts capable of being carried over in vivid ebullition.

To make the experiment in as refined a form as possible, I procured two hollow cones of pure gold containing about 25 grains of water each, they were filled with distilled water, connected together by a moistened piece of amianthus which had been used in the former experiments, and exposed to the action of a Voltaic battery of 100 pairs of plates of copper and zinc of six inches square, in which the fluid was a solution of alum and diluted sulphuric acid. In ten minutes the water in the negative tube had gained the power of giving a slight blue tint to litmus paper: and the water in the positive tube rendered it red. The process was continued for 14 hours; the acid increased in quantity during the whole time, and the water became at last very sour to the taste.

The

Changes produced by electricity in water.

The alkaline properties of the fluid in the other tube, on the contrary, remained stationary, and at the end of the time, it did not act upon litmus or turmeric paper more than in the first trial: the effect was less vivid after it had been strongly heated for a minute; but evaporation and the usual process proved that *some* fixed alkali was present. The acid, as far as its properties were examined, agreed with pure nitrous acid, having an excess of nitrous gas.

I *repeated* the experiment, and carried on the process for three days; at the end of which time the water in the tube was decomposed and evaporated to more than one half of its original quantity; the acid was strong, but the alkali in as minute a portion as in the last experiment. It acted indeed rather more vividly on the tests, on account of the greater diminution of the fluid, but presented the same results after being heated.

It was now impossible to doubt, that the water contained some substance in very minute quantities, capable of causing the appearance of fixed alkali, but which was soon exhausted; and the question that immediately presented itself was, Is this substance saline matter carried over in distillation? or is it nitrogen gas, which exists in minute portions in all water that has been exposed to air, and which, if an element of the fixed alkali, would under the circumstance of the experiment have been soon exhausted, whilst its absorption from the atmosphere would be impeded by the saturation of the water with hydrogen?

I was much more inclined to the former than to the latter supposition. I evaporated a quart of the distilled water that I had used, very slowly at a heat below 140° Fahrenheit, in a silver still; a solid matter remained, equal to $\frac{1}{10}$ of a grain; this matter had a saline but metallic taste, and was deliquescent when exposed to air: I could not obtain from it regular crystals; it did not affect turmeric or litmus, but a part of it, after being heated red, in a silver crucible, exhibited strong alkaline properties. It was not possible to make a minute analysis of so small a quantity, but it appeared to me to be principally a mixture of nitrate of soda and nitrate of lead; and the metallic substance, it is most likely, was furnished by the condensing tube of the common still.

The

The existence of saline matter in the distilled water being thus distinct, it was easy to determine its operation in the experiment. I filled the two gold cones with water in the usual manner; that negatively electrified, soon attained the maximum of its effect upon turmeric paper. I then introduced into it a very minute portion of the substance obtained by the process of evaporation that has been just described; in less than two minutes its effects were evident; and in five minutes the tint of the paper was changed to a bright brown. Changes produced by electricity in water.

I now conceived that by collecting the water obtained in the second process of slow distillation I should be able to carry on the experiment without any appearance of fixed alkali, and the trial proved that I was not mistaken.

Some of this water was introduced into the gold tubes, and the amianthus moistened by it.

After two hours the water in the negative tube produced no effect upon turmeric paper; it did produce an effect upon litmus, which it required great minuteness of observation to perceive; but it wholly lost the power by being heated strongly for two or three minutes, so there is every reason for supposing that it was owing to a small quantity of ammonia.

I made a similar experiment with a portion of the same water in the tubes of agate that had been so often used, and I had the pleasure of finding the results precisely the same.

To detail any more operations of this kind will be unnecessary; all the facts prove, that the fixed alkali is not *generated*, but *evolved*, either from the solid materials employed, or from saline matter in the water.

I have made many experiments in vessels composed of different substances, with the water procured by very slow distillation: and in almost every instance some fixed alkali appeared.

In tubes of wax the alkaline matter was a mixture of soda and potash; and the acid matter a mixture of sulphuric, muriatic, and nitric acids.

In a tube of resin, the alkaline matter seemed to be principally potash.

A cube of Carrara marble of about an inch, having an aperture

Changes produced by electricity in water.

aperture in its centre, was placed in a crucible of platina, which was filled as high as the upper surface of the cube with the purified water, the aperture was filled with the same fluid; the crucible was positively electrified by a strong Voltaic power, and a negatively electrified wire introduced into the aperture.

The water soon gained the property of affecting the tint of turmeric; and fixed alkali and lime were both obtained from it: and this effect took place in repeated experiments: the fixed alkali, however, diminished in quantity every time; and after eleven processes conducted from two to three hours each, disappeared altogether. The production of lime-water was uniform.

I made a solution of 500 grains of this marble in nitric acid; I decomposed the mixture by carbonate of ammonia, and I collected and evaporated the fluid part, and decomposed the nitrate of ammonia by heat. About $\frac{1}{4}$ of a grain of fixed saline matter remained, which had soda for its base.

It was possible that the Carrara marble might have been recently exposed to sea-water; I therefore tried, in the same way, a piece of granular marble, which I had myself broken from a rock on one of the highest of the primitive mountains of Donegal. It afforded fixed alkali by the agency of negative electricity.

A piece of argillaceous schist from Cornwall, treated in the same manner, gave the same result; and serpentine from the Lizard, and grauwacke from North Wales, both afforded soda. It is probable that there are few stones, that do not contain some minute portions of saline matter, which in many cases may be mechanically diffused through their substance: and it is not difficult to conceive the possibility of this, when we consider that all our common rocks and strata bear evident marks of having been anciently covered by the sea.

I was now able to determine distinctly, that the soda procured in glass tubes came principally from the glass, as I had always supposed.

I used the two cones of gold with the purified water and the amianthus; the process was conducted as usual. After a quarter

a quarter of an hour, the negatively electrified tube did not change the colour of turmeric. I introduced into the top of it a bit of glass; in a few minutes the fluid at the surface rendered the tint of the paper of a deep bright brown. Changes produced by electricity in water.

I had never made any experiments, in which acid matter having the properties of nitrous acid was not produced, and the longer the operation the greater was the quantity that appeared.

Volatile alkali likewise seemed to be always formed in very minute portions, during the first few minutes in the purified water in the gold cones, but the limit to its quantity was soon attained.

It was natural to account for both these appearances, from the combination of nascent oxigen and hidrogen respectively; with the nitrogen of the common air dissolved in the water: and Dr. Priestley's experiments on the absorption of gasses by water (on this idea) would furnish an easy explanation of the causes of the constant production of the acid, and the limited production of the alkali: for hidrogen, during its solution in water, seems to expel nitrogen; whilst nitrogen and oxigen are capable of coexisting dissolved in that fluid *.

To render the investigation more complete, I introduced the two cones of gold with purified water under the receiver of an air pump; the receiver was exhausted till it contained only $\frac{1}{24}$ of the original quantity of air; and then, by means of a convenient apparatus, the tubes were connected with an active Voltaic pile of 50 pairs of plates of four inches square. The process was carried on for 18 hours, when the result was examined. The water in the negative tube produced no effect upon prepared litmus, but that in the positive tube gave it a barely perceptible tinge of red.

An incomparably greater quantity of acid would have been formed in a similar time in the atmosphere, and the small portion of nitrogen gas remaining in contact with the water seemed adequate to the effect.

I repeated the experiment under more conclusive circumstances. I arranged the apparatus as before; I exhausted

* Priestley's Experiments and Observations, Vol. I, p. 59.

the

Changes produced by electricity in water.

the receiver, and filled it with hidrogen gas from a convenient airholder; I made a second exhaustion, and again introduced hidrogen that had been carefully prepared. The process was conducted for 24 hours, and at the end of this time neither of the portions of the water altered in the slightest degree the tint of litmus.

It seems evident then, that water chemically pure is decomposed by electricity into gaseous matter alone, into oxigen and hidrogen.

The cause of its decomposition, and of the other decompositions which have been mentioned, will be hereafter discussed.

III. *On the Agencies of Electricity in the Decomposition of various Compounds.*

Action of electricity in decomposing compounds.

The experiments that have been detailed on the production of alkali from glass, and on the decomposition of various saline compounds contained in animal and vegetable substances, offered some curious objects of inquiry.

It was evident, that in all changes in which acid and alkaline matter had been present, the acid matter collected in the water round the positively electrified metallic surface; and the alkaline matter round the negatively electrified metallic surface; and this principle of action appeared immediately related to one of the first phænomena observed in the Voltaic pile, the decomposition of the muriate of soda attached to the pasteboard; and to many facts which have been since observed on the separation of the constituent parts of neutrosaline and metallic solutions, particularly those detailed by M. M. Hisinger and Berzelius *.

The first experiments that I made immediately with respect to this subject were on the décomposition of solid bodies, insoluble, or difficultly soluble in water. From the effects of the electrical agency on glass, I expected that various earthy compounds would undergo change under similar circumstances; and the results of the trials were decided and satisfactory.

* Annales de Chimie, Tom. LI, p. 167.

Two

Two cups made of compact sulphate of lime, containing about 14 grain-measures of water each, were connected together by fibrous sulphate of lime, which was moistened by pure water: the cups were filled with this fluid; platina wires from the Voltaic battery of 100 pairs of plates of six inches were introduced into them, so that the circuit of electricity was through the fibrous sulphate of lime. In five minutes the water in the cup connected with the positive wire became acid; that in the opposite cup strongly tinged turmeric. After an hour the fluids were accurately examined; when it was found that a pure and saturated solution of lime had been produced in the cup containing the negative wire, which was partially covered with a crust of lime; and that the other cup was filled with a moderately strong solution of sulphuric acid. Action of electricity in decomposing compounds.

I procured two cubical pieces of crystallized sulphate of strontites, of about an inch; a hole was drilled in each capable of containing about eight grains of water: the cubes were plunged in pure water in a platina crucible; and the level of the fluid preserved a few lines below the surface of the cubes; two platina wires were introduced into the holes, which were filled with pure water. The disengagement of gas, when the wires were connected with the battery of 100, proved that the sulphate of strontites was sufficiently porous to form a proper conducting chain. The results were much longer in being obtained in this experiment than in the last: some time elapsed before a sensible effect could be perceived; but the termination was similar. In 30 hours the fluid in the cavity containing the negative wire had gained the property of precipitating solution of sulphate of potash; and the presence of sulphuric acid in the other cavity was evident from its effect upon solution of muriate of barytes.

I made an experiment upon fluate of lime under like circumstances; but the crystallized fluate not being equally permeable to moisture, the two cavities were connected by moist asbestus. This decomposition was likewise very slow; but in the course of two days a pretty strong solution of lime was obtained in one tube; and an acid fluid in the other, which precipitated acetite of lead, and left a spot upon the glass from which it had been evaporated.

Sulphate

Action of electricity in decomposing compounds.

Sulphate of barytes, as might be supposed, proved much more difficult of decomposition than either sulphate of strontites or fluate of lime. I had made four or five experiments upon it, with the same kind of apparatus that had been applied to the fluate of lime, before I was able to gain decided results. In the last process performed on this substance, two pieces of a large single crystal were hollowed by grinding, so as to contain about five grains of water each; they were connected by moist asbestas, and constantly subjected during four days to the strong action of a battery of 150 pairs of plates of 4 inches square. As the water diminished, its place was supplied by new quantities. At the conclusion of the experiment the fluid on the positive side of the apparatus instantly reddened litmus, tasted very sour, and gave a distinct precipitate with a solution of muriate of barytes; the water on the other side deepened the tincture of turmeric; but did not render solution of sulphate of potash turbid. There was a small quantity of white crust, however, on the sides and the bottom of the cavity, and I conceived that this might be the barytes, which, during the extremely slow decomposition, would have combined with the carbonic acid of the atmosphere. To ascertain if this had been the case, I introduced into the cavity a drop of diluted muriatic acid; a slight effervescence appeared, and the fluid obtained occasioned a distinct white cloudiness in solution of sulphate of soda.

In all these cases the constituent parts of the bodies newly arranged by the effects of electricity existed in considerable quantities, and exposed on a large surface to its action. I had great reason to believe, from the trials with distilled water in different vessels, that very minute portions of acid and alkaline matter might be disengaged by this agency from solid combinations, principally consisting of pure earths.

This part of the investigation was easily elucidated.

For a purpose of geological inquiry, which on a future occasion I shall have the honour of laying before the Society, I had made a careful analysis of a specimen of fine grained basalt from Port Rush in the county of Antrim,

by

by means of fusion with boracic acid: it afforded in 100 parts $3\frac{1}{2}$ parts of soda, and nearly $\frac{1}{2}$ a part of muriatic acid, with 15 parts of lime. This stone appeared to me very well fitted for the purpose of experiment: cavities were drilled in two pieces, properly shaped; they contained about 12 grains of water each; they were connected by moistened amianthus, and the process conducted as usual with a power of 50 pairs of plates. At the end of ten hours the result was examined with care. The fluid that had been positively electrified had the strong smell of oxi-muriatic acid, and copiously precipitated nitrate of silver; the other portion of fluid affected turmeric, and left by evaporation a substance which seemed to be a mixture of lime and soda. Action of electricity in decomposing compounds.

A part of a specimen of compact zeolite, from the Giant's Causeway, which by analysis had given 7 parts in 100 of soda, had a small cavity made in it; it was immerged in pure water in a crucible of platina, and electrified in the same manner as the cube of Carrara marble, mentioned in page 328. In less than two minutes the water in the cavity had gained the property of changing the colour of turmeric; and in half an hour the solution was disagreeably alkaline to the taste. The matter dissolved proved to be soda and lime.

Lepidolite, treated in the same way, gave potash.

A piece of vitreous lava, from Etna, gave alkaline matter, which seemed to be a mixture of soda, potash, and lime.

As in these trials the object was merely to ascertain the general fact of decomposition, the process was never conducted for a sufficient time to develope a quantity of alkaline matter capable of being conveniently weighed, and of course any loss of weight of the substance could not be determined.

I thought it right, however, to make one experiment of this kind, for the sake of removing every possibility of doubt on the source of the different products; and I selected for this purpose glass, as a substance apparently insoluble in water, and not likely to afford in any way erroneous results.

Action of electricity in decomposing compounds.

The balance that I employed was made for the Royal Institution, by Mr. Fidler, after the model of that belonging to the Royal Society; it turns readily with $\frac{1}{100}$ of a grain when loaded with 100 grains on each side; a glass tube with a platina wire attached, weighing 84 grains $\frac{11}{100}$ was connected with an agate cup, by amianthus; they were filled with purified water, and electrified by a power from 150 pairs of plates, in such a way that the platina in the glass tube was negative. The process was continued for four days, when the water was found alkaline. It gave by evaporation and exposure to a heat of about 400° Fahrenheit, soda mixed with a white powder insoluble in acids, the whole weight of which was $\frac{16}{100}$ of a grain. The glass tube carefully cleaned and dried weighed 84 grains, $\frac{11}{100}$. The difference between the loss of weight of the tube and the weight of the products in the water may be easily explained: some minute detached particles of amianthus were present, and the soda must have contained water, a substance which it is probably perfectly free from in glass.

Having obtained such results with regard to the disengagement of the saline parts of bodies insoluble in water, I made a number of experiments on soluble compounds: their decomposition was always much more rapid, and the phenomena perfectly distinct.

In these processes I employed the agate cups with platina wires, connected by amianthus moistened in pure water; the solutions were introduced into the cups, and the electrifying power applied from batteries of 50 pairs of plates, in the usual way.

A diluted solution of the sulphate of potash treated in this manner, produced in four hours at the negative wire a weak lixivium of potash; and a solution of sulphuric acid at the positive wire.

The phenomena were similar when sulphate of soda, nitrate of potash, nitrate of barytes, sulphate of ammonia, phosphate of soda, succinate, oxalate, and benzoate of ammonia, and alum were used. The acids in a certain time collected in the tube containing the positive wire, and the alkalies and earths in that containing the negative wire.

Solutions of the muriatic salts, decomposed in the same way, uniformly gave oximuriatic acid on the positive side.

When

When compatible mixtures of neutrosaline solutions containing the common mineral acids were used, the different acids and the different bases seemed to separate together in a mixed state, without any respect to the orders of affinity. Action of electricity in decomposing compounds.

When metallic solutions were employed, metallic crystals or depositions were formed, as in common galvanic experiments, on the negative wire, and oxide was likewise deposited round it; and a great excess of acid was soon found in the opposite cup. With solutions of iron, zinc, and tin, this effect took place, as well as with the more oxidable metals: when muriate of iron was used, the black substance deposited upon the wire was magnetic, and dissolved with effervescence in muriatic acid; and when sulphate of zinc was used, a gray powder possessed of the metallic lustre, and likewise soluble with effervescence, appeared; and in all cases acid in excess was exhibited on the positive side.

Strong or saturated saline solutions, as might have been expected, afforded indications of the progress of decomposition much more rapidly than weak ones; but the smallest proportion of neutrosaline matter seemed to be acted on with energy.

A very simple experiment demonstrates this last principle. If a piece of paper tinged with turmeric is plunged into pure water in a proper circuit, in contact with the negative point, the very minute quantity of saline compound contained in the paper affords alkaline matter sufficient to give it instantly a brown tint near its point of contact: and acid in the same manner is immediately developed from litmus paper, at the positive surface.

I made several experiments, with the view of ascertaining whether, in the decompositions by electricity, the separation of the constituent parts was complete from the last portions of the compound; and whenever the results were distinct, this evidently appeared to be the case.

I shall describe one of the most conclusive of the experiments: a very weak solution of sulphate of potash, containing 20 parts water and one part saturated solution, at 64°, was electrified in the two agate cups by the power of 50 pairs

Action of electricity in decomposing compounds.

50 pairs of plates for three days: the connecting amianthus, which had been moistened with pure water, was removed, washed with pure water, and again applied, twice every day; by this precaution the presence of any neutral salt that might adhere to it, and disturb the results, was prevented. The alkali obtained in this process in the solution had the properties of pure potash; and when it had been saturated with nitric acid it gave no turbidness by mixture with solution of muriate of barytes: the acid matter exposed to a strong heat evaporated without leaving any residuum.

IV. *On the Transfer of certain of the constituent Parts of Bodies by the Action of Electricity.*

Transference of certain constituent parts of bodies by electricity.

M. Gautherot has stated*, that in a single galvanic circle of zinc, silver, and water, in an active state, the oxide of zinc formed is attracted by the silver †; and M. M. Hisinger and Berzelius detail an account of an experiment, in which solution of muriate of lime being placed in the positive part of a siphon, electrified by wires from a Voltaic pile, and distilled water in the negative part, lime appeared in the distilled water.

These facts rendered it probable, that the saline elements evolved in decompositions by electricity, were capable of being transferred from one electrified surface to another, according to their usual order of arrangement; but to demonstrate this clearly, new researches were wanting.

I connected one of the cups of sulphate of lime, mentioned page 331, with a cup of agate by abestus; and, filling them with purified water, made the platina wire in the cup of sulphate of lime transmit the electricity from a power of 100; a wire in the agate cup received it. In about four hours a strong solution of lime was found in the agate cup, and sulphuric acid in the cup of sulphate of lime. By reversing the order, and carrying on the process for a similar time, the sulphuric acid appeared in the agate cup, and the solution of lime on the opposite side.

Many trials were made with other saline substances, with

* Annales de Chimie, Vol. XXXIX, page 203.

† Ibid. Vol. LI, page 171.

analogous

analogous results. When the compounds of the strong mineral acids with alkaline or alkaline-earthy bases were introduced into one tube of glass, distilled water connected by amianthus being in another tube, both connected by wires of platina in the Voltaic arrangement, the base always passed into the distilled water when it was negative, and the acid when it was positive. Transference of certain constituent parts of bodies by electricity.

The metals and the metallic oxides passed towards the negative surface like the alkalies, and collected round it. In a case in which solution of nitrate of silver was used on the positive side, and distilled water on the negative, silver appeared on the whole of the transmitting amianthus, so as to cover it with a thin metallic film.

The time required for these transmissions (the quantity and intensity of the electricity, and other circumstances remaining the same) seemed to be in some proportion as the length of the intermediate volume of water. Thus when, with the power of 100, sulphate of potash was on the negative side, and distilled water on the positive side, the distance between the wires being only an inch, sulphuric acid, in sufficient quantity to be very manifest, was found in the water in less than five minutes: but when the tubes were connected by an intermediate vessel of pure water, so as to make the circuit eight inches, 14 hours were required to produce the same effect.

To ascertain whether the contact of the saline solution with a metallic surface was necessary for the decomposition and transfer, I introduced purified water into two glass tubes; a vessel containing solution of muriate of potash was connected with them respectively by amianthus; and the arrangement was made in such a way, that the level of both the portions of purified water was higher than the level of the saline solution.

In this case, the saline matter was distant from each of the wires at least $\frac{3}{4}$ of an inch; yet alkaline matter soon appeared in one tube, and acid matter in the other: and in 16 hours moderately strong solutions of potash, and of muriatic acid had been formed.

In this case of electrical transfer or attraction, the acid and alkaline matter seemed to be perfectly pure; and I am

Transference of certain constituent parts of bodies by electricity.

inclined to believe, that this is uniformly the case in all experiments carefully made. One of the instances in which I conceived acid most likely to be present, was in the transfer of magnesia from sulphate of magnesia in the positive tube, to distilled water in the negative tube. I examined the case, taking care that the distilled water was never upon a lower level than the saline solution: the process was continued for some hours, till a considerable quantity of magnesia had appeared. The connecting amianthus was removed, and muriatic acid poured into the tube: the saturated solution did not precipitate solution of muriate of barytes.

I endeavoured to ascertain the progress of the transfer, and the course of the acid or alkaline matter in these decompositions, by using solutions of litmus and turmeric, and papers coloured by these substances; and these trials led to the knowledge of some singular and unexpected circumstances.

Two tubes, one containing distilled water, the other solution of sulphate of potash, were each connected by amianthus with a small *oz.* measure filled with distilled water tinged by litmus: the saline solution was negatively electrified; and as it was natural to suppose, that the sulphuric acid in passing through the water to the positive side would redden the litmus in its course, some slips of moistened paper tinged with litmus were placed above and below the pieces of amianthus, directly in the circuit. The progress of the experiment was minutely observed; the first effect of reddening took place immediately above the positive surface, where I had least expected it; the red tint slowly diffused itself from the positive side to the middle of the vessel, but no redness appeared above the amianthus, or about it, on the negative side, and though it had been constantly transmitting sulphuric acid, it remained unaffected to the last.

The order of the experiment was changed, and the saline solution placed on the positive side; a solution and papers tinged with turmeric being substituted for those tinged with litmus. The effect was precisely analogous; the turmeric became brown first near the negative wire, and no change took

took place in the intermediate vessel near the positive wire.

In another process, the two glass tubes were filled with solution of muriate of soda, and the intermediate vessel with solution of sulphate of silver: paper tinged with turmeric was placed on the positive side, and paper tinged with litmus on the negative side; as soon as the electrical circuit was complete, soda began to appear in the negative tube, and oximuriatic acid in the positive tube, and the alternate products were exhibited passing into the solution of sulphate of silver, the muriatic acid occasioning a dense heavy precipitate, and the soda a more diffused and a lighter one; but neither the turmeric transmitting the alkali, nor the litmus transmitting the acid, had their tints in the slightest degree altered.

(To be continued.)

II.

Extract of a Letter from Mr. J. M. Haussmann *to Mr.* Bertholle[t]*.

WHEN I published my memoir on Stahl's alkaline tincture of steel, I imagined that this superoxigenated sulphate of iron would not fail to be examined more minutely: but finding myself disappointed in this respect, I cannot refrain from again calling to mind some of its properties that appeared to me striking. These are its being wholly insipid to the taste, when completely deprived of its water of crystallization: and of acquiring astringent powers, surpassing those of any other astringent known, as soon as it has imbibed moisture from the air, or been dissolved in water. This superoxigenated sulphate of iron produces the most beautiful prussian blue possible; and it may be used with advantage in dyeing, particularly for blacks.

Superoxigenated sulphate of iron.

Its properties.

I read with pleasure Mr. Thenard's paper, but I cannot be of his opinion with respect to the nitrate of iron saturated with oxigen, crystals of which I can easily produce

Nitrate of iron crystallized.

* Annales de Chimie, Vol. LVIII. p. 182, May, 1806.

without diluting the nitric acid of 40° of Messrs. Coustos and Co. at Paris. I use this same acid, to convert sugar into oxalic acid; and, whether I employ it of its full strength, or diluted with equal parts of water, I constantly obtain a little greasy matter, when I conduct the process in the large way on a vapour bath.

Fatty matter from sugar.

Sugar treated with nitric acid.

On treating the same sugar three times successively with equal portions of this acid, either concentrated or diluted, the first portion occasions a brown colour, and produces a smell of burnt sugar. And when the action of the nitric acid has ceased, we already perceive some of this grease swimming at the top; and it appears to be farther increased by the successive addition of the other two portions of acid, which cause the brown colour and smell of burnt sugar to disappear, forming a great abundance of oxalic acid, and a small quantity of the malic and citric acids. Perhaps, if the gasses were collected, we should find a little acetic acid also.

No oil from it by boiling.

To satisfy myself whether the sugar gave rise to the formation of the grease, I examined one of the largest sized sugar-loaves, which I commonly use. I divided it into two equal portions, the first consisting of the outer part of the loaf, the second of the inner. Each of these portions I boiled for a few minutes in three times its weight of water. No grease swam on either of these solutions of sugar, after they were cold: but as they were not very clear, I began to suspect, that, the sirup for common sugar being clarified with bullock's blood by the sugar bakers, the gelatinous part of this animal substance unites in some measure with the particles of sugar by a forced and confused crystallization, and, when acted upon by nitric acid, may give rise to the separation of grease. I was not long before I satisfied myself, that my suspicion was just, for, on making oxalic acid with some fine white sugarcandy, and at the same time with the finest loaf sugar I could procure, neither of these showed any signs of grease.

Probably from the fining.

None from fine sugar or candy.

Fat oils separated from soaps by an acid acquire a solvent power.

Fat oils in their natural state have not the least action on asphaltum, jews' pitch, or copal: but if they be reduced to a soap, and afterward separated by any acid, they not only exert a strong solvent power on these substances.

stances, but they further acquire the property of decomposing acetate of lead, as well as other metallic acetates, and of combining readily with their oxides, the acetic acid of which is given out. These oils thus separated would produce the same effect perhaps on other metallic salts. In general all fats, resins, and turpentines, combine better with other substances, after they have been reduced to soap and separated by an acid, than in their natural state. Wax comports itself in the same manner. A knowledge of this effect induced me to subject to the action of the process for forming oxalic acid a small portion of oil separated from Marseilles soap, which I mixed with sugar previously powdered. At the end of the operation I found, that the oil had acquired the consistence of suet, and that it had assumed a yellowish colour and a rancid smell, retaining the property of swimming on water. This grease, having been exposed to the same process a second time, had its rancidity increased, contracting at the same time a little of the smell of wax; and its specific gravity became so great, that, after it had been well washed and perfectly freed from acidity, it sank to the bottom of water, without having lost its property of being soluble in alcohol.

This common to fats and resins.

This oil exposed to the action of nitric acid.

My memoir on indigo shows, that I had long ago built great hopes on the action of nitric acid with respect to other substances, and it is with great pleasure I perceive, that Messrs. Fourcroy and Vauquelin have pursued my researches on indigo exposed to the action of nitric acid with more success than I obtained. I could only have wished, that Mr. A. Laugier had passed me over in silence in his abstract of the paper of those learned chemists, for my way of thinking in chemistry is totally different now from what it was eighteen years ago. When Mr. Laugier quoted me, he should not have forgotten that passage in my paper, which mentions the results of treating indigo with nitric acid, results that struck me so forcibly, as to induce me to recommend them to the attention of chemists. Neither had I omitted to mention the phenomena of the deflagration of the mixture, with the throwing of the glass rod out of the evaporating vessel. As a little time before I undertook these experiments I had extracted the benzoic

Indigo.

acid

acid from its gum, I was too well acquainted with its smell, not to have distinguished it in purifying and drying the residuums of indigo treated with nitric acid, if my occupations as a manufacturer, which prevent me from gratifying my inclination for chemical experiments, had not proved an obstacle. Perhaps too I should not have missed the discovery of the detonating property of the bitter portion of the residuum: but it seems I was not born to make a figure in the career of discovery.

Arsenical alkaline solution of indigo.

With respect to the solution of indigo by means of an alkaline solution of red arsenic, which is used in calico printing, I no longer observe the proportions indicated in my memoir. I simply make a caustic alkaline solution of red arsenic, to which I add, while it is yet boiling, a sufficient quantity of brayed indigo, to obtain a very deep shade, which it is easy to render lighter afterward, according to the object proposed, by diluting the solution of indigo with a weak lixivium of caustic potash. This is preferable to pure water, because it retards in some measure the absorption of oxigen from the atmosphere, and consequently the regeneration of the indigo. The beauty of the blue in the calicoes requires, that this regeneration should be neither too slow nor too speedy. The too slow absorption arising from too great excess of caustic alkali ought to be avoided in pencilling blues, as well as in the blues in block-printing, which are procured by passing the goods, first printed with brayed indigo mixed with a gummy solution of sulphate of iron, alternately through vats of caustic potash, water, sulphate of iron at a minimum of oxidation, and lastly a vat acidulated by sulphuric or muriatic acid.

Cautions respecting its application.

Indigo and muriate of tin.

On exposing to a sand-heat a mixture of brayed indigo with a muriatic solution of tin oxided at a minimum, in which there is an excess of acid, the colouring substance is decomposed, occasioning the evolution of a gas of an insupportable and noxious smell, which deserves to be examined.

Sulphate of indigo and muriate of tin.

If indigo treated with the muriatic solution of tin oxided at a minimum, without the assistance of a caustic alkali, cannot be of any use in dyeing, it is not the same with sulphate

sulphate of indigo, treated or mixed in different proportions with the same solution of tin, after having previously absorbed sulphuric acid. This is employed in the manufacture of printed goods for producing all sorts of blues and greens.

III.

Observations on the Distilled Water of common Borage; by PHILIP ANTONY STEINACHER, *Member of the Pharmaceutic Society of Paris* *.

HITHERTO no particular property had been observed in borage water, except its depositing mucous filaments after being kept some time. Some that I distilled on the 7th of June, 1806, exhibited the following remarkable properties. The borage was very fresh, succulent, and immediately after being very finely shred was put into the body of a tinned copper alembic. Two parts of distilled water were poured on it, which moistened it sufficiently. The head of the still was put on, and a receiver adapted to it, both of which were previously rinsed clean with distilled water. The distillation was commenced immediately with a heat so gentle, that 20 or 30 seconds intervened between the fall of the successive drops. Only half a part of water was drawn off, which was limpid and colourless, and smelt and tasted strongly of borage, at the same time having another smell resembling that of a cucumber.

Borage water

very carefully distilled.

This water neither reddened litmus paper, nor turned green paper tinged with an infusion of red roses; but it perceptibly reddened an aqueous infusion of litmus, which had been diluted with distilled water so as to appear of a pure blue.

Reddened infusion of litmus.

It rendered lime-water turbid instantly.

Precipitated lime.

A few drops of pure rectified sulphuric acid, distilled almost to dryness, and diluted with distilled water, produced after the expiration of a few minutes a disengagement of

With sulphuric acid effervesced

* Annales de Chimie, Vol. LX, p. 83, October, 1806.

some

some very small bubbles, without emitting any nitrous, muriatic, or acetous smell, and without disturbing its transparency.

Action of other tests.

It instantly whitened the aqueous solutions of oxalate of ammonia, muriate of barytes, nitrate of lead, and sulphate of silver *.

Oxigenized muriate of mercury converted by it into mild muriate.

The oxigenized muriate of mercury, purified by slow sublimation, produced in it a copious white precipitate. At the expiration of half an hour I added lime-water in excess, which increased the quantity of the precipitate, and did not turn it yellow even in twenty-four hours; which it would infallibly have done, if the oxigenized muriate of mercury had not been converted into muriate at a minimum.

With acid sulphate of mercury gave an opal precipitate.

Finally, having mixed with it some acid sulphate of mercury in a liquid state, made by dissolving the yellow sulphate in sulphuric acid, the addition of caustic potash purified by alcohol separated from it in a quarter of an hour flocks of an opal colour; while the same alkali, added to the same sulphate of mercury without borage water, immediately formed in it yellow flocks †.

Principles contained in it.

Hence it follows, that my borage water, distilled with so much care, and by a heat so gentle, contained carbonic acid, sulphate of lime, and a disoxiding principle.

Sulphate of silver decomposed by that of lime.

* Sulphate of silver is decomposed by the action of sulphate of lime. The following experiment is a direct proof of this. I took some very limpid lime-water, and added a few small drops of pure sulphuric acid. The solution remained clear, and had an excess of acid. To this I added a little of my acid sulphate of silver, which immediately occasioned a flocculent precipitate. After this had been washed, it was not soluble in muriatic acid. This fact proves, that the oxide of silver enjoys a very considerable power of cohesion, and renders me very circumspect in forming a judgment of the experiments for deciding the presence of muriatic acid in delicate fluids from the single phenomenon of precipitation by means of any solution of silver, and without examining the other circumstances, that might occasion the insolubility of the oxide of silver.

Forms an insoluble oxide, without muriatic acid.

Erratum in Fourcroy's Chemistry.

† According to Fourcroy's Chemistry, the sulphate of mercury with excess of oxide is precipitated gray by the alkalis; but this must be an errour of the press, our illustrious professor having before shown, that this property belonged to the neutral sulphate of mercury, which he had discovered.

I must

I must observe, that several parcels of borage, gathered on different soils, and not so fresh, did not exhibit the same phenomena in an equal degree. As to this disoxiding principle, which has a sensible effect on mercurial solutions, I have found it in several other distilled waters, particularly in the water of silver weed, *potentilla anserina*, and strong scented lettuce, *lactuca virosa*. The water of the last-mentioned plant holds in solution besides a fetid volatile oil, which is rendered visible by adding rectified alcohol at 37°. It is no wonder therefore, that these distilled waters are capable of producing some effect in the art of dyeing*: they must be of use likewise in some sthenic diseases.

Some borage produced less.

Other distilled waters contain the same principle.

Wild lettuce water.

Hence their use in dyeing, and in some diseases.

IV.

A Memoir on Acetic Acid; by Mr. J. B. Trommsdorff †.

The object of Mr. Trommsdorff was, to know whether azote make a part of the acetic acid, as Proust asserted. Having considered what is at present known respecting the composition of ammonia, and of vegetable acids, he was justly surprised to find in Mr. Proust's paper, that he had found ammonia and prussic acid in decomposing acetates. Accordingly, notwithstanding the known accuracy and sagacity of the chemist of Madrid, he was desirous of satisfying himself of the existence of azote in concentrated acetic acid. The importance of the fact, and a love of truth, led this indefatigable chemist to make a similar research.

Nitrogen said to be one of the principles of acetic acid by Proust.

Before relating the processes Mr. Trommsdorff employed, it may be proper to give a succinct statement of the objections, that occurred to him.

If, says he, in the distillation of acetates ammonia be formed, it is evident that they contain azote; but whence

Objections.

* On consulting the anecdotes that Mr. Deyeux has published on distilled waters, in No. 108 of the Annales de Chimie, it appears, that he found the distilled water of silver-weed had a decided action on the silks that he used to make gauze.

Distilled waters act on silk.

† Annales de Chimie, Vol. LVIII, p. 190, May, 1806.—Abridged from the Berlin Journal by Mr. Bergman.

can

can this principle be derived? Is it from the base? But this cannot be, since ammonia was equally obtained from acetate of lead. In this case it could be furnished only by the acetic acid; or it must be allowed, that azote is only a modification of hidrogen.

Does nitrogen occur in vegetable acids?

He then inquires whether azote occur as frequently among vegetable acids, as among animal acids: because, if it be so, their classification should be altered.

Very pure substances employed.

In repeating the experiments of Mr. Proust, it appeared essential to Mr. Trommsdorff, to employ only very pure substances. Accordingly, in order to have acetic acid in the purest state possible, he decomposed the acetate of potash by sulphuric acid; he then saturated this acid with carbonate of soda well purified, and evaporated the saline solution in a silver basin. The salt obtained, which he put into a bottle with a ground stopper, was extremely white.

In preparing the acetates of potash and of lead he employed similar precautions.

These distilled.

He took eight ounces of each of these salts, and introduced them separately into three strong glass retorts. These retorts were placed on the open fire of a furnace, and to each was adapted a receiver, from which issued a glass tube, terminating under a jar for receiving the gasses that should come over.

The products.

The products were, as every body knows, an acidulous ethereous fluid mixed with oil. The alkali and carbone remained in the retort; and in the decomposition of the acetate of lead nothing of this salt remained but the lead oxided.

The alkalis promoted the decomposition of the acid.

Thus by the predisposing affinity of the alkalis for carbonic acid, these determined the decomposition of the acetic acid, to give rise to the formation of carbonic acid. The oxide of lead on the contrary, not having so great an affinity for carbonic acid, gave out the acetic acid in its greatest purity.

How does the metal act?

The author asks, whether the metallic base yielded up oxigen to burn the carbone; or whether the attraction between an oxide and an acid be less powerful than between an acid and an alkali. To answer this question, he would wish a great number of experiments to be made.

The

The gasses on examination emitted no ammoniacal smell, it being merely empyreumatic and penetrating. The liquids had the same smell, and none of the chemical tests could detect the presence of ammonia in them. No ammonia in the gasses or liquids,

The residuums, which according to Mr. Proust contained prussiate, were nothing but pure alkaline carbonate; or pure oxide of lead. or in the residuums.

Mr. Proust, on examining the residuum of acetate of potash, says, that he had a residuum consisting in part of prussiate, in part of carbonate of potash. Mr. Trommsdorff expected to find these two salts; but, after having broken the retort, he found only a homogeneous coal, which afforded him neither ammonia nor prussic acid, and which had no smell of either of these substances. Yet we know how easy it is to distinguish the smell of this acid wherever it exists in a free state. From the acetate of potash Proust obtained prussiate and carbonate.

Mr. Proust adds, that the residuum of the acetate of potash was so saturated with prussic acid, that its bitterness was as striking as if the acid had been combined directly with the alkali; whence Mr. Trommsdorff infers, that he must have employed common vinegar in his experiments. The prussic acid in the residuum very apparent.

To distinguish the products resulting from the decomposition of acetic acid, Mr. Trommsdorff passed the vapour of it through a red hot tube, which afforded him nothing but carbonic acid gas, carburetted hidrogen gas, and a small quantity of an empyreumatic liquor, without ammonia, and without prussic acid. These substances were equally absent in the residuums. Vapour of acetic acid passed through a hot tube.

He afterwards examined attentively the ethereous aciduous fluids mingled with oil. These he distilled over carbonate of potash, and obtained an ether, which, from all its properties, appeared to be true acetic ether. Acetic ether obtained by rectifying the liquors.

The results of his experiments are:

1. That the presence of azote in acetic acid is not proved. General conclusions.

2. That pure acetates, when distilled, give out neither ammonia nor prussic acid.

3. That pure acetic acid has its nature very little altered by passing through red hot [glass] tubes.

4. That, in an iron tube, it is completely decomposed into carbonic acid gas, and carburetted hidrogen gas.

5. That

Constituent principles. 5. That the constituent parts of acetic acid are demonstrated to be oxigen, carbone, and hidrogen.

Acetic ether. 6. That the ethereous fluid is similar to others in its general properties. The author considers it as a medium between alcohol and ether. That as acetic acid is changed in part into ether, and this, when treated by nitric acid, is transformed into oxalic acid: the conversion of acetic acid into oxalic is demonstrated, though it is true indirectly. And lastly, that it is probable Mr. Proust did not employ pure acetates in his experiments; or else the ethereous and very penetrating smell led him to believe, that ammonia was present.

Acetic acid convertible into oxalic.

V.

Account of an Engine for splitting Sheep Skins: by Mr. Benjamin Stott, *of Bermondsey Street* *.

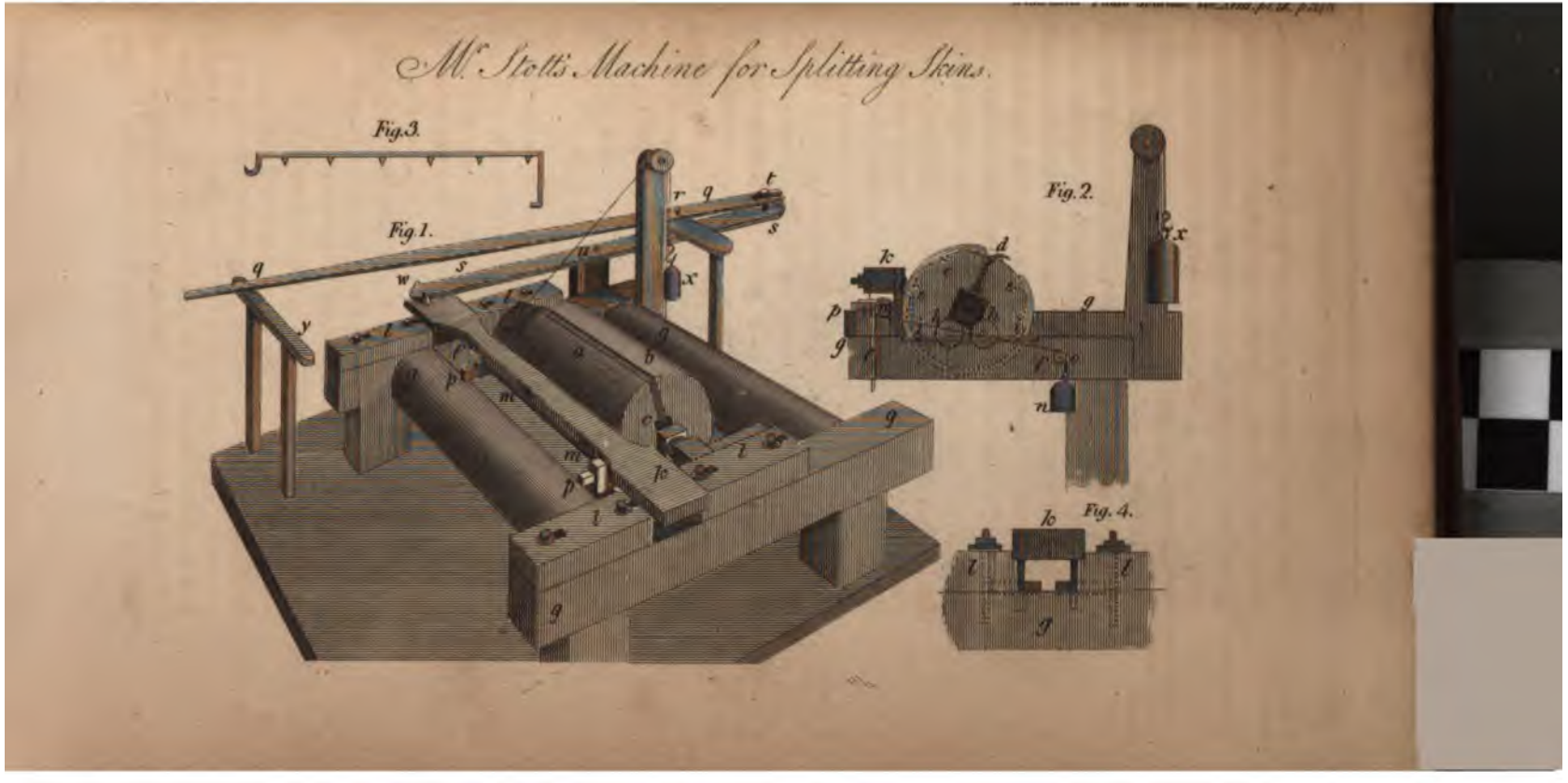
Advantages of an engine for splitting skins. I HAVE invented an engine for the purpose of splitting sheep skins, that is, of making two good skins out of one. The former and common mode of dressing skins is, to shave one side off, reserving the shavings for glue pieces; whereas by my method, these shavings are all taken off in one piece, forming a good skin of leather; and thus, independently of the advantage arising to the proprietor, an additional revenue will be caused to the nation, in proportion to the increase of leather made.

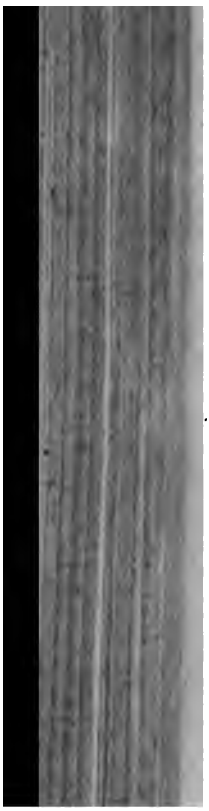
Description of the engine. Pl. IX, Fig. 1. A, the barrel of cast iron (having wooden ends) round which barrel the skin is wrapped, and kept close by means of pins run through the edges into the wood, as at *c*, *c*, Fig. 2. B, (Fig. 1) an iron running in a groove along the barrel, catching in a hole at c, and fastened down at the other end by a hook fixed in the end of the barrel, the bar having points in it (as shown at B, Fig. 3,) under which the edges of the skin are fastened (as seen at D. Fig. 2). F, F, (Fig. 1 and 2) bars fixed across each end of the strong wooden frame G, G, G, G, over which the barrel is supported on friction-rollers, as at *h*, *h*, (Fig. 2) which

* Transactions of the Society of Arts, vol. xxiv, p. 133. The Society voted Mr. Stott twenty guineas for this invention.

run

Mr. Stotts Machine for Splitting Skins.





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ASTOR, LENOX AND
TILDEN FOUNDATIONS
R L

run on a slip of brass, moveable under the screws *i*, *i*, to adjust the barrel to the knife. K, K, (Fig. 1 and 2) a strong bar of cast iron, to which the knife is screwed, moving lengthwise on friction-rollers between the pieces of wood L, L, L, L, on the frame G, as at K, (Fig. 4). The pieces of wood L, L, L, L, are each moveable under two screws, by which they are adjusted to steady the motion of the knife-bar. M, M, (Fig. 1 and 2) is a roller at the back of the knife, to which it is kept close by a weight N, at each end, acting over pulleys, as at O, (Fig. 2) suspended from the slider *p*, between which the roller is placed; by drawing the spare skin over this roller, as it is cut off, it keeps both sides of the skin equally up to the knife, and makes it cut more uniform. Q, Q, is a lever acting on a pin *r*, and moving another lever S, S, by means of a pin and a notch *t*, which acts on another pin at *u*; and by means of the two pins at W it moves the knife lengthwise to and fro: as fast as the skin is cut the barrel is drawn round by the weight X. *y* is a guide to the lever, from which end it is worked.

VI.

A Memoir on Sulphuric Acid; by Mr. KLAPROTH: *read at the Philomathic Society of Berlin* *.

THE object of the author was to ascertain the respective quantities of the elements of sulphuric acid and of sulphate of barytes, and he mentions the analyses that have been made of these substances by the chemists whose names are subjoined: Proportions of the elements of sulphuric acid according to various authors.

Sulphuric acid consists,	Sulphur.	Oxigen.
according to Lavoisier, of	69	31
Berthollet -	72	28
Thenard - -	55·56	44·44
Chenevix -	51·5	38·5
Trommsdorff	70	30
Richter - -	42·05	57·95
Bucholz -	42·5	57·5

* Abridged from the German by Mr. Bergman. *Annales de Chimie*, Vol. LVIII, p. 122, May, 1806.

The

The last two, though calculate
nearest to each other, and therefor
But Mr. Klaproth conceived it ne
by his own experiments of the re
elements of sulphuric acid, that
the results with more certainty to
metallic sulphurets. For this p
other chemists had done, nitric aci

Proportion of the elements of sulphate of barytes.

tes; the elements of this salt havi
tained by him to be barytes ·78, c
Mr. Bucholz, however having
salt consists of ·79 barytes, and
Klaproth repeated his analysis w
still obtained the same proportion
add, that Mr. Bucholz admits on
bonate of strontian, while Mr. K
all the analyses he has made of thi
The results of the analysis of
made by various chemists do not d
tions they have assigned to the con
phuric acid. It is composed,

Given differently by different authors.

According to Fourcroy, of	
Clément and Desc	
Thenard	-
Chenevix	
Kirwan	-
Richter	
Bucholz	-

Kirwan's preferred by Klaproth.

If indeed we except the analyses
the rest do not vary greatly fro
take a mean of these, Kirwan's c
has induced Mr. Klaproth to adop

Process for deciding the question.

Proceeding on these data, the
adopted by Mr. Klaproth. He
pure sulphur, and eight ounces o
specific gravity of 1·32, into a retc
three fourths had passed over into
duct of this distillation was retu
distilled a second time. Eight o
then added, and the whole distille

The unburned sulphur was found to weigh 48 grains and half; consequently 151½ were converted into sulphuric acid. The product diluted in a certain quantity of water was mingled with muriate of barytes, till no more precipitate was produced. The sulphate of barytes, well washed and dried, weighed 1109 grains; but, calcined in a platina crucible, its weight was reduced to 1082 grains.

To find the proportions of the constituent principles of concrete sulphuric acid, Mr. Klaproth took a hundred grains of highly concentrated sulphuric acid, the specific gravity of which was 1·85: this he diluted with fifteen parts of water, and added to it muriate of barytes, till no more precipitate was formed. The sulphate of barytes, carefully washed and dried, weighed 225 grains. Hence it follows:

Proportions of highly concentrated sulphuric acid:

1st, that 100 parts of sulphuric acid of the specific gravity of 1·85 are composed of

Concrete sulphuric acid	74·4	or, sulphur	31·5
Water - -	25·6	oxigen	42·9
		water	25·6
	100		

2dly, That 100 parts of concrete acid are formed of sulphur 42·3, oxigen 57·7. of concrete acid:

3dly, That 100 parts of calcined sulphate of barytes contain barytes 67, sulphur 14, oxigen 19. and of sulphate of barytes.

SCIENTIFIC NEWS, &c.

A Classification of Vegetables, and Plan of a new Method formed on that of Tournefort, according to which the Plants of the Garden of the private School of Pharmacy at Paris are arranged: by D. L. GUYART, *Assistant Professor of Botany at the School, &c.*

IF, among the different botanical methods, that of Tournefort has always been considered as the most easy, and best calculated to guide the first steps of those who would study plants; it must also be confessed, that it is insufficient, when we endeavour to obtain an accurate idea of vegetable organization. For this reason, no doubt, the methods of Linneus and Juspieu are at present preferred, and almost universally adopted by botanists. Tournefort's classification of plants.

It

Improvement of his method.

It is of importance, however, that Tournefort's should not be lost, as well on account of the celebrity of its author, as for the utility of which it may still prove to young students. By these motives Mr. Guyart has been induced to compose a new classification of vegetables, founded on the method of Tournefort; but in which, availing himself of the progress subsequently made in the science of botany, he has formed his classes from more striking and constant characters than those adopted by Tournefort. Thus he has given fresh youth to the method of that botanist, and rendered it more natural.

M. Guyart's arrangement.

Tournefort's new method, as proposed by Mr. Guyart, consists of sixteen classes. The first eight are formed of plants with complete simple flowers. The first containing the monopetalous: the second, the personate: the third, the labiate: the fourth, the cruciform: the fifth, the rosaceous: the sixth, the umbelliferous: the seventh, the caryophyllaceous: the eighth, the leguminous. The next three include the plants with complete compound flowers, with united anthers: the semifloscular, the floscular, and the radiate. The four following are appropriated to the distinct incomplete flowers: the apetalous, the amentaceous, the glumaceous, and the liliaceous. The sixteenth and last is assigned to the anomalous plants, or those with indistinct incomplete flowers.

Useful to beginners and those who wish for a general knowledge merely.

This classification, as the author observes, is not free from defects; but, notwithstanding its imperfections, almost unavoidable, perhaps, in such an undertaking, in the opinion of some botanists of celebrity, whom he has consulted, it will much facilitate the study to beginners, and is still better adapted to those, who, not having time to cultivate the science to its full extent, require only an acquaintance with its elements.

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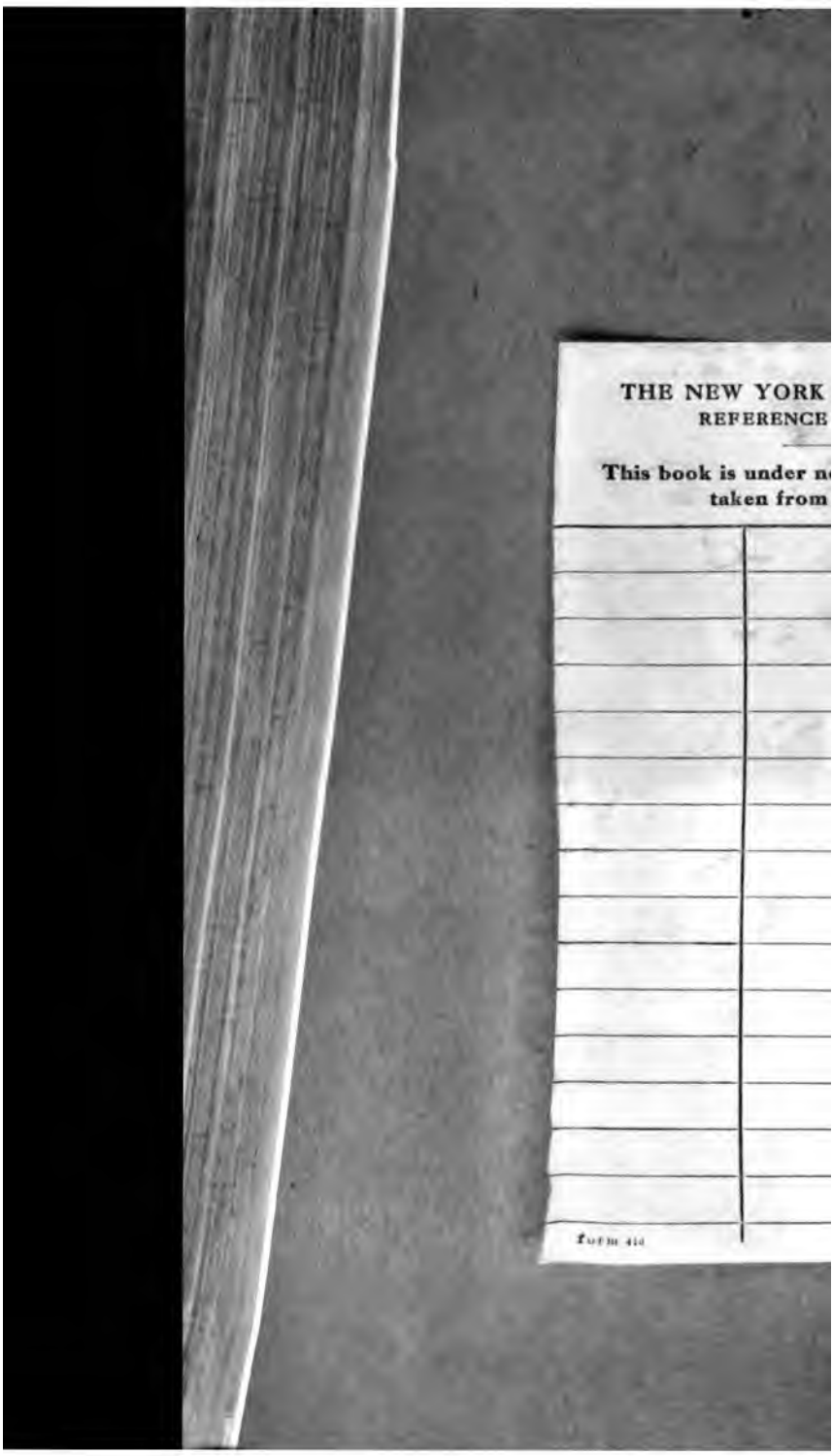
Printed by W. Stratford, Crown-Court, Temple-Bar.

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